Peer Effects in Police Use of Force

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Abstract

We study the link between officer injuries-on-duty and their peers' force-use using a network of officers who attended the police academy together through a random lottery. Peer injuries-on-duty increase the probability of using force by 7% in the subsequent week. Officers are also more likely to injure suspects and receive complaints about neglecting victims and violating suspects' constitutional rights. The effect is concentrated in a narrow time window following the event and is not associated with significantly lower injury risk to the officer. Together, these findings suggest that officers' emotional responses drive the increase in use of force and misconduct.

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1. INTRODUCTION

Excessive use of force by police is a critical social and economic issue in the United States. Aggressive policing results in human capital and educational losses among affected populations (Legewie and Fagan, 2019*a*; Ang, 2021), in addition to the hundreds of police killings each year (Lartey, 2015) and significant costs to taxpayers through misconduct settlements (Thomson-DeVeaux, Bronner and Sharma, 2021). The social cost of police behavior and tactics has led the Biden Administration to view addressing systemic misconduct as a key policy objective.[1] However, there is little empirical evidence on the factors contributing to officers' use of force. In this paper, we show that peer effects, injuries on-duty, and negative emotional shocks can play a significant role in an officer's decision to use force.

We use detailed administrative data on the Chicago Police Department (CPD) to show that shortly after a peer is injured, officers are more likely to use force and injure civilians. Identifying such peer effects is generally difficult due to the nature of social and professional networks: officers may choose peers who have similar preferences for the use of force. Also, officers who work together likely face common risks of injuries and needs to use force. Furthermore, identifying an officer's network is generally difficult. We overcome these challenges by exploiting the fact that the CPD police academy draws in new officers using random lottery numbers– meaning officers do not have a choice in their academy peer group– and, after training is complete, new officers are generally sent to different areas across the city. We denote these officers who went to the academy together but who work in

[1] https://www.whitehouse.gov/briefing-room/speeches-remarks/2021/04/20/remarks-by-president-biden-on-the-verdict-in-the-derek-chauvin-trial-for-the-death-of-george-floyd/.

different areas as "former peers."[2] Former peers provide us with an observable network of peers which is not chosen by the officer and who do not face the same local shocks to use of force or injury risk. We exploit the exogenous timing of a former peer's injury using a difference-in-differences design which compares officers who had a former peer injured to their contemporaneous peers (working in the same district) who did not have a former peer injured.[3]

We find that a former peer's injury during a use of force incident increases an officer's propensity to use force by 7% in the subsequent week, leading to a 10% increase in the likelihood of injuring a civilian. Officers are also more likely to receive a complaint about a false arrest or improper search in the week following a former peer's injury. These findings suggest that officers respond to the injury of a peer by violating suspects' constitutional rights. We also find a higher probability of receiving a complaint about the failure to provide service, suggesting officers sort out of helping potential victims.

These results likely understate the full effect of officer injuries for two reasons. First, the estimation strategy excludes peers in the same geographic police district because these officers may experience correlated shocks to civilian non-compliance. Injuries to these contemporaneous peers are likely to be at least as impactful due to plausibly lower social distance between the coworkers relative to former peers. For example, when we consider another

[2] Shue (2013) and Ager, Bursztyn and Voth (2017) use a similar definition for their peer groups.

[3] Identifying the effect of an officer injury on a peer's decision requires exogeneity in the officer's injury with an assignment probability independently distributed across groups (Angrist, 2014). We approximate this ideal using an approach similar to a partial population design (Duflo and Saez, 2003; Hirano and Hahn, 2010) by combining random assignment of officers to groups with a difference-in-differences design exploiting exogenous timing of officer injuries.

group with plausibly lower social distance – cohort members of the same race – the effect doubles in magnitude. Second, we drop the first year of data for all officers in our sample because we cannot identify the officer's geographic district during that time. Effects are likely larger during this period because officers have less experience and are likely closer to their academy peers.

To better understand these effects, we document mechanisms driving peer use of force. We find evidence consistent with officers using force due to an emotional response as the increase in police violence is immediate and quickly disappears two weeks after an injury. This pattern is consistent with other studies on negative emotional shocks leading to short-term increases in violence (Card and Dahl, 2011; Munyo and Rossi, 2015). Furthermore, as in Guryan, Kroft and Notowidigdo (2009), we find that professional experience attenuates social influences such as peer effects, and this moderating effect of tenure is similar to Ta, Lande and Suss (2021) who find that a police officer's emotional reactivity is lower in more experienced police officers. We also rule out several alternative explanations: we find no evidence that officer's are simply 'mimicking' their former peer's use of force nor do they reduce their own injury likelihood through social learning or updated beliefs. Lastly, by linking officers to arrest records, we rule out the possibility that they are reducing their effort (Mas (2006), Ba and Rivera (2019)).

This article makes contributions to three literatures. First, it contributes to the growing literature on police discretion by documenting that police decision-making is influenced by peer injuries. Much of the existing literature on crime and policing focuses on crime prevention and incapacitation effects[4], while a growing body of work has focused on police as individual

[4]See Levitt (2002), Di Tella and Schargrodsky (2004), Evans and Owens (2007), Draca,

agents which exercise discretion resulting in differences in outcomes such as arrests, stops, and the use of force.[5] More recent studies have documented how aggressive policing can reduce the educational performance of minority groups, negatively affect attitudes toward the state and undermine police legitimacy.[6] By identifying a causal determinant in the decision to use force, this paper builds upon the burgeoning literature attempting to unpack the black box of police productivity by providing evidence that police officers respond to their peers' outcomes.[7] These findings introduce a new dimension for policymakers to consider. Policies that increase the risk of injury to officers will have a muted effect on force-use when officers respond to risk by increasing force. Alternatively, policies that reduce the risk to officers may have positive externalities on force-use.

Second, it contributes to the literature on peer effects by documenting evidence that individuals respond to peer outcomes rather than choices in the workplace (Mas and Moretti, 2009; Cornelissen, Dustmann and Schönberg, 2017). This result suggests that direct responses to peer outcomes may partially drive the results in other studies that find negative spillovers. For example, Carrell and Hoekstra (2010) find negative spillovers from children in troubled families and argue that these effects operate through the reduced achievement or increased disruption of the affected child. Similarly, Murphy (2019) attributes contemporaneous misconduct in the military to

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Machin and Witt (2011), Chalfin and McCrary (2018), and Morales (2020) for papers documenting the effect of policing on crime.

[5]Knowles, Persico and Todd (2001); Lum and Nagin (2017); Fryer (2020); Knox, Lowe and Mummolo (2020); Ba et al. (2021)

[6]See Skolnick and Fyfe (1993), Tyler (2004), Weitzer and Tuch (2004), Brunson and Miller (2005), Lum and Nagin (2017), Manski and Nagin (2017), Legewie and Fagan (2019*b*), and Ang (2021).

[7]See Fryer (2018), Owens et al. (2018), Ba and Rivera (2019), Ba et al. (2021), Annan-Phan and Ba (2019), and Zimring (2019) for other work attempting to uncover the determinants of police force.

peers responding to the poor behavior of other soldiers. Our finding provides new mechanisms for exploring such results. We also confirm that one's peer group can affect individuals' choices and outcomes long after the group dissipates (Bayer, Hjalmarsson and Pozen, 2009; Shue, 2013).

Third, this paper contributes to the literature on the effects of exposure to violence by showing that a peer's exposure to violence affects an individual's behavior in high-stakes decisions. Lab and artefactual field experiments have uncovered evidence that exposure to violence can increase preferences for certainty and impatience while decreasing emotional regulation.[8] Outside of the lab, Bauer et al. (2016) find that people exposed to war behave more cooperatively and altruistically towards their ingroup. In contrast, we find that those exposed to violence behave less altruistically towards outgroup members. Similarly, Hjort (2014) find that ethnic conflict increases animus against outgroup members and Aizer (2009) finds that exposure to violence can reduce future productivity.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background, providing information on the network formation and the policy governing use of force decisions. Section 3 describes the relevant datasets, sample definitions, and summary statistics. Section 4 explains the research design used to generate the estimates provided in Section 5. Section 6 sheds light on the mechanisms suggested by auxiliary data analysis. Section 7 concludes.

[8]See Callen et al. (2014), Imas, Kuhn and Mironova (2018), Moya (2018), Brown et al. (2019) for lab and artefactual field evidence that violence can affect preferences and behavior. Osofsky (1997) finds that violence can decrease emotional regulation.

2. BACKGROUND

2.1 FORMATION OF POLICE NETWORKS

The Chicago Police Department's (CPD) recruitment process creates an ideal setting to study spillover effects. The recruitment process generally follows five steps: (1) a recruitment call[9], (2) an entrance exam, (3) a referral lottery, (4) a battery of physical and mental tests, and finally (5) the officer attends the police academy.

The CPD regularly issues recruitment calls. Table A1 displays the nine recruitment calls made between 2002 and 2013. After applying, prospective officers take an exam meant to evaluate the officer's cognitive and non-cognitive abilities. In step 3, the CPD adds all of the applicants who pass the exam to an eligibility list and assigns each a lottery number. These applicants are referred to the CPD academy in lottery order as vacancies become available, with veterans receiving priority in the randomization. Applicants remain on the lottery list until it is either exhausted or retired. This application process ensures that individuals do not select into specific cohorts based on their propensity to use force, be injured, or respond to peer injuries with violence.

Our data contain officers' start dates and the dates that the CPD held recruitment tests. However, we do not observe an individual officer's test (the entrance exam referred to in step 2) date as the CPD is unable to supply this information (see Appendix A.3 for more information). In its place, we use the date of the most recent test before the officer started at the academy as a

[9]Officers who apply to be part of the Chicago Police Department must fulfill age and citizenship requirements. Applicants must also have a combination of post-secondary and/or army training: applicants must have at least sixty semester hours from an accredited university, three years of active duty in the armed forces or thirty semester hours, and one continuous year of active duty (Department, n.d.).

proxy for their test date. We overcome any measurement error in our proxy test dates by using individual fixed effects in our main specifications.[10]

Applicants whose lottery numbers are called proceed to step 4, where they must pass further examinations to proceed. These include a physical test, a background check, a psychological evaluation, and a drug test. After the officer passes these examinations, they start at the police academy. We refer to all officers starting the academy together in the same month as an academy cohort. Figure 2 shows the dates these cohorts start at the police academy throughout the sample period. On average, police academy cohorts are 78% male, 49% white, 17% Black, and 34% Hispanic. The median age of new officers in our sample is 28 years. Figure 3 presents a histogram of the cohort sizes during the sample period; cohorts have on average 42.81 individuals, though cohort sizes range considerably.

Once applicants enter the academy, the Education and Training Division provides over 900 hours of basic training over six months. Training includes instruction on use of force tactics, including firearms and control techniques. There is also physical and scenario-based training in the classroom. CPD recruits receive additional training on gangs, drugs, law, ethics, report writing, vehicle stops, and driving.

After completing the academy, officers complete roughly twelve months of probationary field training.[11] During the twelve months of field training,

[10] These individual fixed effects allow us to overcome this issue because whatever date the officer took the test is time-invariant and subsumed by the individual fixed effect.

[11] Nearly all officers who begin training graduate from the police academy with fewer than 3% of officers failing (Hinkel (2017)). The probationary period consists of eighteen months of active duty. Officers spend the first six months in the academy and the final twelve months in probationary district assignments. Time absent from duty does not apply toward completion of the probationary period.

the CPD assigns probationary officers to districts at their discretion. Duty assignments can change day-to-day during this period. Unfortunately, the unit assignment data does not record probationary assignments. After the probationary period is over, officers move to more permanent police units, based on the needs of the CPD rather than the preferences of the police officer. As discussed earlier, we define an officer's "former peers" as the members of their academy cohort who are working in a different geographic unit in a given week.

Most police officers work in geographic units which are tasked with policing a specific geographic area, known as a police district. In 2013, there were 22 police districts in Chicago, with officers often being assigned to police specific beats (about one square mile) within the district.[12] We focus our analysis on these geographic units. In an average week, we observe 94 officers who joined between 2002 and 2013 in each of the 22 units. The distribution of unit sizes is shown in Figure 4, and each unit consists of many different cohorts (see Figure 5). In an average week, officers have 60 former peers.

2.2 USE OF FORCE POLICY

The CPD defines force as physical contact by a department member used to compel a subject's compliance. It is the department's policy that officers should attempt to gain the voluntary compliance of subjects when possible. However, officers are not required to take actions that endanger themselves or third parties (Chicago Police Department, General Order G03-02).[13]

[12] A smaller share of officers work in specialized units, such as Canine, Marine/Helicopter, SWAT, or Bomb Squad units. Since these specialized units operate across geographic districts, we omit them from the analysis.

[13] When attempting to gain the compliance of subjects, officers have several options available to them. Officers can use mitigation efforts such as verbal directions to gain compliance without using force. They may also use control tactics such as handcuffing or applying

Under Chicago Police Department General Order G03-02, the CPD requires officers to use force that is “objectively reasonable, necessary, and proportional” to the subject’s actions. However, there is no formal definition of “objectively reasonable.” The CPD instructs officers to consider whether there is an imminent threat to themselves or third parties, how much harm the threat poses, and whether the subject has immediate access to weapons. When assessing the validity of force, the CPD explicitly accounts for imperfect information regarding the suspect’s compliance and that officers make decisions quickly under tense circumstances.

The requirement of proportional force relies on the officer’s contemporaneous beliefs about the threat she faces. These beliefs may differ from those determined by an objective observer. The guidelines permit deadly force when the officer believes that the suspect poses an imminent threat of great bodily harm. Officers may also use deadly force when the suspect committed a forcible felony threatening the infliction of great bodily harm and attempted to avoid arrest. Under the guidelines, the department only permits officers to use this type of force as a last resort when all other de-escalation methods have failed.

Despite these guidelines, a 2015 probe by the Justice Department found that the CPD engaged in a ”pattern or practice” of unconstitutional use of force. The Justice Department’s report noted that CPD officers regularly engage in behavior that endangers themselves, resulting in unnecessary and avoidable force-use. It attributed the unconstitutional force to deficiencies in account-

pressure to sensitive areas. Officers are also permitted to use higher-level responses with or without weapons; these include open hand strikes, punches, kicks, and other forms of physical violence. Lastly, the CPD permits officers to use Tasers, pepper spray, batons, and firearms under some circumstances.

ability and insufficient support for officers' wellness and safety. The findings further noted that the unconstitutional force fell most on Black and Latino neighborhoods. See Press Release 15057 (2017) for more details.

3. DATA AND SUMMARY STATISTICS

We use four sources of administrative data from the Chicago Police Department. Use of force and injury data come from the Chicago Police Department's Tactical Response Reports (TRR) for non-juvenile suspects. The CPD requires that officers fill out Tactical Response Reports after an officer uses more than a minor level of force.[14] While minor levels of force do not require a TRR, officers must also fill out TRRs when a suspect alleges an injury, if the suspect resists arrest or in situations where the suspect uses physical violence. (Chicago Police Department General Order G03-02-02).

Our data encompass over 16,000 instances of force by the CPD between January 1, 2005 and October 31, 2016. These data have numerous strengths relative to other existing data sets. They cover almost every instance of police use of force in Chicago, regardless of whether the officer injures or kills the suspect.[15] Second, the data contain detailed information about the time and location of the incident along with suspect, officer, and interaction characteristics. We supplement the data with officer employment records that include unit assignments and report the officer's start date, because it is critical

[14]This includes firearms, impact munitions, Tasers, acoustic devices, impact weapons, mechanical actions/techniques, or chemical weapons. Minor levels of force include things like holds or handcuffing. The CPD also requires TRRs for force involving canines, but we exclude canine units from the analysis.

[15]The data exclude incidents involving juveniles and subjects with unknown ages because juvenile records are not subject to Freedom of Information Act requests.

to our identification strategy.[16]

To help understand how a former peer's injury influences officer behavior, we supplement this data with data on complaints issued against officers and data on arrests.[17] The complaint data contain all allegations of misconduct filed by civilians or other officers from January 1, 2005 until June 17, 2016, including the date of the incident and details of the actions resulting in a complaint. We match this data to the officer data using the complainant's self-reported incident date. We investigate three specific types of complaints: force and verbal abuse, improper search or arrest, and failure to provide service. The arrest data contain all CPD arrests of adults during our sample period, including crime type, arrestee demographics, and arrest date and time (see Appendix A.2 for more details).

The data do have some limitations. While we observe the presence of any alleged injury to officers or civilians, we do not observe the nature, severity or cause of the injury.[18] We restrict our treatment definition to injuries that occur during interactions with suspects who allegedly attacked the officer.[19] While it is unlikely an officer is injured by a violent suspect but reports the suspect as non-aggressive, misclassification may occur when an officer

[16]We exclude all police officers without a recorded start date from the analysis because we cannot link these officers to a police academy cohort.

[17]See Ba (2017) for a detailed discussion of this data.

[18]The CPD refused to provide this information in the FOIA request citing HIPPA privacy regulations.

[19]The data does not include information about the nature or extent of officer injuries. However, Tiesman et al. (2018) reports that the most common cause of injuries-on-duty is violence. Most of these injuries were to the hands, legs, neck, head, or shoulders. About 40% of injuries were contusions, abrasions, lacerations, fractures, or dislocations. The other 60% were sprains, strains, or other. In their sample, assault-related injuries grew between 2003 to 2011. The Bureau of Labor Statistics also reports that of the 27,660 on-the-job injuries reported in their 2014 sample, violence caused 27% of injuries. The next most common category was falls, slips, and trips; this category accounted for 25.3% of injuries. Overexertion followed, accounting for 21.4% of injuries.

is injured due to an accident and a suspect was aggressive (i.e., the injury was not actually caused by the suspect). This misclassification would lead to measurement error, as we will consider control periods (no former peer was injured due to a suspect) as treated periods (at least one former peer injury due to a suspect); however this attenuates our effects as long as the covariance between former peer injuries and measurement error does not exceed the variance of the measurement error itself (Aigner, 1973; Black, Berger and Scott, 2000).

Lastly, CPD officer unit assignment data records officers as part of the academy unit until they finish their probationary period rather than graduate from the academy. We use the sample of officers who start at the academy after January, 2001. We cannot observe the officers' geographic assignments in the year between graduation and the end of the probationary period. Since local non-compliance shocks constitute a significant threat to identification, we exclude every officers' probationary year from the analysis. We drop officers who leave the academy before six months or who graduate after our sample period.

A total of 4,429 officers start the academy between 2001 and 2013 (see Table 1), and we study these officers between 2004 and 2016. We drop 967 officers who do not enter into a geographic district after leaving the academy, leaving us with 3,462 officers (see Table 2) and a total of 986,111 officer-week observations between 2004 and 2016 (the time period for which we observe TRRs). Of these officers, 2,836 use force at least once in the sample, with 2,000 instances accompanying an injury or alleged injury to the suspect. In our sample, 1,280 officers experience injuries. Nearly all officers (3,429) experience at least one injury to a member of their police academy cohort.

Table 3 displays the summary statistics of events and outcomes by week. An individual officer is very unlikely to be injured in a given week. However, 90% of weeks involve at least one officer injury and 10% of weeks involve an injury to a former peer. Officers use force in about 1.8% of weeks. When they use force, they tend to use more than one type. Officers arrest suspects in about 26% of weeks and get complaints for their actions in about 1.6% of weeks. We show the correlation between outcomes, events and characteristics in Table 4. Figure 1 displays the positive correlation between officer injuries in a week and other officers' force-use.

3.1 VERIFYING RANDOM COHORT ASSIGNMENT

We verify the random assignment of officers to cohorts using the procedure from Guryan, Kroft and Notowidigdo (2009) to correct for the mechanical negative correlation between an officer's characteristic and that of their peer group.[20] In our case, this takes the form

$$X_{igr} = \pi_0 + \pi_1 \bar{X}_{-i,gr} + \phi \bar{X}_{-i,r} + \epsilon_{igr}. \tag{1}$$

Here, X_{igr} is the average pre-determined characteristic of officer i in academy cohort g chosen from test cohort r. We approximate the test cohort using cohorts which begin between two test dates.[21] The pre-determined characteristics we observe are the officer's sex, age at appointment, and race, and

[20]Guryan, Kroft and Notowidigdo (2009) corrects the procedure in Sacerdote (2001) to show that there is a mechanical negative correlation between one's characteristics and that of its peer group arising from the fact that peers are, in a sense, sampled without replacement. That is, peers with high values of a characteristic are chosen from a group with slightly lower mean characteristics than those with low values because removing them from the group reduces the mean of the set. They also show that this bias is decreasing in the size of the set. Randomization in our setting occurs on the entrance-exam cohort level. Therefore, the randomization sets we consider range from 52 to 1,300 officers (see Table A1).

[21]We do not know for certain whether officers in two cohorts come from the same test cohort as the CPD does not track this information (see Appendix A.3 for more details). In our research design, we overcome this issue by using officer level fixed effects.

the descriptive statistics for these variables appears in Table 1.

We regress X_{igr} on $\bar{X}_{-i,gr}$, the class cohort leave-out-mean of X_{igr}, and $\bar{X}_{ir}$, the mean of X_{igr} for all individuals in test cohort r. Estimates of Equation 1 for each of the pre-determined observables is presented in Table 5. The lack of statistical significance on the class cohort leave-out-mean coefficients suggests that officers are not assigned based on race or sex. We find a positive correlation between an officer's age and the age of their class. However, this difference is economically small. A one-year increase in the average age of a class cohort is associated with a half a year increase in an officer's age. Overall, these tests suggest that there is random assignment to police academy cohorts in practice. The discrepancy in the age variable may be due to a preference granted to Veterans in the lottery process.

4. RESEARCH DESIGN

The empirical analysis aims to identify the causal effect of an injury-on-duty in an officer's network. The ideal research design uses two stages of randomization: individuals are first randomized into a group, then, within each group, individuals are further randomly selected into treatment or control conditions (Philipson, 2000; Duflo and Saez, 2003; Hirano and Hahn, 2010; Crépon et al., 2013; Angrist, 2014; Baird et al., 2018). Following Ager, Bursztyn and Voth (2017), we approximate this design by first choosing to define our peer group as officers who, through the random lottery, attended the police academy together and now work in a different district.[22] We then exploit the quasi-random timing of force-related injuries to those former peers.

[22]This means that we consider individuals to be untreated if a member of their academy cohort who currently works in the same district is injured.

In principle, the relevant network is the set of officers whose injury status is observable to the officer in question, which could include all officers or officers in the same unit. However, our definition helps alleviate concerns that we are identifying correlated effects and not peer effects, wherein the correlation in officer injuries and force-use is due to officers who are more likely to get into altercations with suspects also being more likely to be in the same group. More aggressive officers sorting into more aggressive networks could spuriously generate a correlation between injuries and use of force that could be mistaken for a peer effect in the data. The CPD recruitment process ensures that each group's unobservable characteristics, such as the officers' aggression, are balanced across academy cohorts within each test cohort.

Our definition also addresses potentially confounding common shocks to civilian non-compliance, which we assume are district-specific. When some shock reduces the probability that civilians comply with an officer's requests, then both the risk to the officer and the returns to using force will increase. Using *former* peers allows us to rule out district-level shocks to civilian non-compliance because we compare treated officers to other officers in their own district who face the same non-compliance rate.

Finally, we use the panel structure of the data in conjunction with a difference-in-differences design to eliminate bias arising from both types of simultaneity. We use lagged peer injuries because contemporaneous peer injuries may be confounded by contemporaneous events that affected the non-compliance rate for all parties in that week, and it may also take time for officers to learn about the injuries of their peers. Moreover, Angrist (2014) shows that designs relying on random variation in cohort assignments do not overcome the reflection problem because identification relies on finite-sample fluctuations in treatment assignment. As such, we use the quasi-random timing of injuries to former peers as an approximation of the random assignment of injuries within an academy cohort.

In practice, we construct the counterfactual outcomes within district d and week t, using injuries to former peers as the treatment. We identify the effect of an officer injury using an event study that compares individuals experiencing and not experiencing an injury to a former peer and combining them into a difference-in-differences estimator. The event of a former peer's injury occurs at time $t = E_i$ for individual i. We denote individual fixed effects as λ_i and district-week fixed effects as λ_{dt}. The primary equation used to recover the causal effect of peer injuries is

$$Y_{idgt} = \lambda_i + \lambda_{dt} + \beta \cdot \mathbb{1}[t = E_{g,-d} + 1] + \epsilon_{idgt}. \tag{2}$$

Here, the unit of observation is an officer-week. The outcome variable, Y_{idgt}, is an indicator function, equal to one if the outcome is realized for officer i working in district d during week t who belongs to academy cohort g. For example, in our main specifications, Y_{idgt} is equal to one if officer i chose to use force in week t and zero otherwise. An advantage of this approach is that we include officer-week observations in which the officer did not use force, make an arrest, etc., which allows us to avoid endogeneity issues from conditioning on interactions with police (Ba et al. (2021)).[23] The treatment, $\mathbb{1}[t = E_{g,-d} + 1]$, is an indicator equal to 1 if a former peer (an officer who attended the police academy with officer i and is working in a different district) was injured in the previous week.

The individual fixed effects, λ_i, account for time-invariant individual-level

[23] See Fryer (2018), Heckman and Durlauf (2020), and Fryer (2020) for discussions of this issue.

differences in the outcome. For example, they account for time-invariant differences in how each officer interprets a suspect's actions as non-compliance. They also subsume test-cohort fixed effects which is the level of randomization to the peer group.
District-week fixed effects, λ_{dt}, account for district-week level differences in the costs and benefits of choosing $Y_{idgt} = 1$. These fixed effects control for district-specific shocks to civilian or officer aggression, such as the weather or pollution (Herrnstadt et al., 2016; Annan-Phan and Ba, 2019), and control for common shocks under the partial interference assumption.

The coefficient of interest, β, estimates the change in the outcome for affected officers relative to officers in the same district who did not experience a former peer injury in the previous week. Standard errors are clustered on the academy cohort level to allow for arbitrary correlation of errors within each of the 81 cohorts. The main identifying assumption is that the change in the outcome in a given district-week is independent of whether an injured officer started the police academy in the same month as officer i.

To assess this assumption's plausibility and examine the dynamic effects of a peer injury, we regress the outcomes on lags and leads of injuries to a former peer. We denote event time in this regression as τ. We omit the dummy for the week before a former peer is injured so that we can interpret the coefficients relative to the week before the injury. We set period -6 to be equal to one when the event was six or more weeks before the injury and set period 6 to be one if the week is six or more weeks after the injury.[24]

[24]The lags and leads will also alter the composition of individual-weeks that we observe. The first and last six weeks of every individual's observations in the panel will be excluded from the regression since the end periods combine many additional time periods, and this adds noise to the estimates of the effects in those periods.

$$Y_{idgt} = \lambda_i + \lambda_{dt} + \sum_{\tau} \beta_\tau \cdot \mathbb{1}[t = E_{g,-d} + \tau] + \epsilon_{idgt}, \quad (3)$$

where $\tau = \{-6+, -5, -4, -3, -2, 0, 1, 3, 4, 5, 6+\}$. In this regression, the coefficients of interest are now β_τ. These coefficients estimate the change in the outcome between period $t = -1$ and τ for officers who experienced a peer injury relative to members of the same district who did not. Insignificant β_τ estimates before the event alleviate concerns that the groups differ in the probability of encountering non-compliant civilians or differ in the way they interpret signals during interactions with civilians.[25]

Officers experience multiple events over their time in our sample. Standard event studies usually include one event per cross-sectional unit and mutually exclusive dummy variables representing periods before and after treatment. Our setting departs from this standard. While the probability an individual officer gets injured is one quarter of one percent, over 95% of weeks in our sample contain at least one officer injury, and officers have roughly a one in eight chance of experiencing a peer injury each week. Over the observed portion of an officer's career, the average officer experiences 0.89 injuries, 43.62 injuries to former peers, and 368.48 injuries to any police officer. Thus, β_τ can represent the effect for a period, which is both a pre-treatment period and a post-treatment period. Assuming the response to treatment does not vary based on the number of previous events, this will bias the pre-trend estimates away from zero and make it more likely for us to find significant pre-trends. However, in nearly all specifications, we do not find evidence of pre-trends.

[25]However, unobserved post-treatment shocks specific to members of a particular cohort may still threaten the identification of β_τ. For example, if an officer is injured it may mean a loss of manpower which leads to other officers working more, thus mechanically increasing their potential for use of force. However, since former peers do not work in the same district as the injured officer, this is unlikely to threaten our identification strategy.

At present, there is no accepted method of conducting event studies when there are multiple or overlapping events. The Monte Carlo simulation results in Sandler and Sandler (2014) suggest that allowing multiple event dummies to be non-zero at one time produces unbiased results under a similar data generating process. Further, they show that restricting the estimation to consider only a single event or using only periods that have a single event per individual, event, or time produces biased estimates of the treatment effect. We follow their guidance in our estimation and allow multiple event dummies to be non-zero.

5. RESULTS

This section demonstrates that after a former peer is injured during a violent interaction, officers substantially increase their propensity to use force in a short time period. Officers are also more likely to injure suspects and receive complaints about their conduct during the first two weeks after an injury to a former peer. Officer tenure reduces the magnitude of these effects. Moreover, the effects are moderated by social distance, as officers respond twice as strongly to the injury of former peers of the same race.

5.1 PROPENSITY TO USE FORCE AND INJURE SUSPECTS

We first consider whether officers increase their propensity to use force after a former peer is injured, to avoid the reflection problem ((Manski, 1993)) and local shocks to civilian non-compliance. Table 6 displays the results of estimating Equation 2 with the outcome being the use of force and each column containing different controls and specifications. Column (1) displays a strong correlation between officer injuries and the propensity to use force.

Former peer injuries are associated with a 24% higher likelihood of using force in the following week.

However, to establish a causal relationship, we must address three main threats to identification. First, we account for local time-varying shocks to civilian non-compliance by adding district-week fixed effects in Column (2). Second, we account for heterogeneity in cohort size that may have affected the quality of teaching and the probability of experiencing an event in Column (3). Next, we account for sorting into more aggressive networks using our test-period proxy fixed effects in Column (4).[26] Finally, in Column (5) of Table 6, we replace test fixed effects with individual fixed effects. These fixed effects allow us to control for time-invariant differences in an individual's propensity to use force and overcome any mis-classification of test dates.[27] We find that the treatment effects do not change substantially or qualitatively with the introduction of individual fixed effects.

Estimates in Columns (4) and (5) are nearly identical, with a former peer's injury increasing an officer's likelihood of using force in the following week by 7% relative to the baseline rate. Furthermore, the R^2 numbers in these regressions increases from 0.024 to 0.040, suggesting that the individual level fixed-effects do a good job absorbing unobserved differences between individuals. Finally, because the outcome is rare (the probability an officer uses force is under 2% per week), we include a Poisson specification as a robustness check in Column (6).[28] The coefficient in Column (6) of Table 6 can be

[26] Changes in the applicant pool may lead to cohorts that systematically differ from one another in unobservable ways, making them more likely to experience injuries and use force.

[27] One's test cohort is time-invariant and is therefore perfectly collinear with the individual fixed effect.

[28] We choose to estimate the model using Poisson maximum likelihood estimation rather than with a Logit model because of the incidental parameters problem (Neyman and Scott,

readily interpreted as an approximation of the percent change. We find this coefficient is similar to the OLS estimate in size and significance.

To better understand the consequences of the increases in force-use, we estimate Equation 2 with a suspect injury as the outcome variable in Table 7, with the same controls and specifications as in Table 6. Column (1) of Table 7 shows that the baseline rate of suspect injury per week is 0.53%. Based on Column (5), injuries to former peers increase an officer's propensity to injure suspects by 10% of this baseline mean. As in Table 6, comparing Columns (4)-(6) shows that these results are similar when replacing test-period fixed effects with individual-level fixed effects or using a Poisson regression.

5.2 EVENT STUDY

Next, we investigate the dynamics of the treatment effect and test whether there are parallel pre-trends between officers with and without a former peer injured. To do so, we estimate Equation 3 where the outcome variable is whether the officer used any force in the week after a former peer is injured.[29] We estimate the base rate of force as the constant term from a regression of Equation 3 without individual or unit-week fixed effects. Figure 6 displays the coefficient estimates from Equation 3 divided by the base rate. This allows us to interpret the effects as percent changes from the baseline propensity to use force.

The effects of a former peer injury in the weeks before the injury are nei-

1948; Hahn and Newey, 2004). Wooldridge (1999) shows that Poisson maximum likelihood works well with a binary response variable.

[29]As cohorts join throughout the sample period, the estimation equation will drop individuals who are not employed for five weeks before and after an exposure. As a robustness check, Column (1) in Appendix Table A4 repeats this exercise using a Poisson regression specification.

ther economically nor statistically significant. Baseline use of force is also small, with 1.78% of officers using any force in a given week. In the week of a former peer injury, the use of force increases by around 3% of the baseline mean. We view this period as partially treated as some officers experience injuries toward the beginning of the week, while the weeks after the injury are fully treated as injuries may happen toward the end of the week and it may take a few days for officers to learn of the injury.

In the week after a former peer is injured, officers increase their use of force by roughly 7% of the baseline mean. The treatment effects dissipate quickly, immediately losing significance after the first-week after exposure. This suggests that the effect of the an officer injury is fleeting and concentrated around the event. Similar effects are displayed in Figure 7 by estimating Equation 3 with suspect injuries as the outcome.

5.3 TYPES OF FORCE USED BY OFFICERS

Next, we investigate what types of force officers used to respond to the injury of former peers. The CPD's use of force model governs the choice of which type of force to use, and the level of force an officer is permitted to use increases with the level of resistance they face.

The lowest level of force is called control tactics. It includes actions such as escort holds, wrist locks, emergency handcuffing, and armbars. Above that is physical strikes, such as takedowns, open hand strikes, punches, kicks, or elbows, which do not involve more than the officer's body. The CPD classifies force involving weapons as non-lethal if it involves a chemical weapon and classifies other force involving a weapon by weapon type (i.e., baton or impact weapon, Taser, or firearm). We categorize all other uncommon types of force as ”Other.”

We estimate Equation 2 on indicators for using each type of force separately and present the results in Table 8. The majority of instances of force recorded in this data involve force with no weapon, control tactics, and Tasers; the rarest type of force is the use of firearms, followed by impact weapons such as batons. Note that there is likely much lower under-reporting for types of force that are harder to conceal (firearms and Tasers) because of the multiple CPD regulations for reporting their usage.

Similar to our main results, we do not find evidence of pre-trends in any specification except for non-lethal force. We find that officers primarily respond to a former peer's injury by increasing control tactics and force without weapons, by about 8% relative to baseline for both. There is a substantial increase in officers using a firearm in the week after a former peer is injured, amounting to a 48% increase relative to baseline. However, this represents a very small percentage point increase due to the rarity of firearm usage. Assuming that officers are using force in alignment with the CPD's use of force model, the increase in force-use is primarily driven by encounters with low-resistance suspects, suggesting that officers would not have deemed these suspects to be a risk had their peer not been injured in the previous week.

5.4 SAME-RACE FORMER PEERS

Thus far, our definition of former peer has included all officers who attended the academy with an officer and now work in other police districts. For peer injuries to have an effect, these individuals must have been acquainted and maintained their bonds after the academy ended. Given that even randomly assigned groups still produce homophilic friendships (Carrell, Sacerdote and West, 2013), we expect stronger bonds to be between individuals of the same race (Marmaros and Sacerdote, 2006). Such homophily will attenuate effects in previous results, as we will be pooling strongly and weakly treated officers. Following McPherson, Smith-Lovin and Cook (2001), we assume individuals of the same race who attended the academy together are more likely to be a part of the same network. We perform the same analysis as before, but we now define a (former) peer group as members of the same academy cohort now working in different districts who are all same race.

Table 9 and Figure 8 repeat our analysis from the previous section using this definition of peer groups. We find that officers respond twice as strongly to the injury of a same-race former peer. Officers increase their propensity to use force by 16.5% in the week after a same-race former peer is injured with a similar baseline probability of using force. This result is in line with existing literature that shows peer effects mainly operate within race (Garlick, 2018).[30]

5.5 INJURIES UNLIKELY TO BE CAUSED BY SUSPECTS

As a robustness check, we investigate officers' propensity to respond to former peers' injuries that occurred during interactions with suspects who displayed low levels of resistance– i.e., when a former peer reported that the suspect did not attack them during the encounter. While we do not know the cause of these injuries, the results of Tiesman et al. (2018) suggest that they may be due to falls, slips, or trips. If officers respond to a perceived threat, we would expect officers to not respond with increased force-use to these types of injuries.

[30] We conduct the same analysis for officers of the same gender (see Table A14) and find the effects to be only slightly larger than the main results.

Table 10 and Figure 9 display estimates of the effect of these injuries on an officer's propensity to use force. In line with our expectations, we find no evidence that officers respond to injuries of former peers that were unlikely to be caused by suspects.

5.6 COMPLAINTS

Next, we investigate the effect of injuries to former peers on allegations of misconduct by the officers. When a police officer acts outside the confines of the U.S. Constitution or other relevant regulations, there are limited ways to detect this violation. Any civilian who believes an officer violated their constitutional rights may sue the law enforcement agency or report the violation to an oversight agency. Complaints are generally easier to file than lawsuits and are the most readily available form of civilian feedback a police department can access (Walker and Macdonald, 2008; Ba, 2017; Ba and Rivera, 2019).

In this section, we consider whether officers are more likely to act in a way that generates a complaint in the week following a former peer's injury. Table 11 and Figure 10 show estimates of Equations 2 and 3 respectively with an indicator for having any complaint as the outcome. We find that in the week following a former peer injury, officers are 7% more likely to engage in behavior that leads to a complaint of any type (Column (1)).

We then consider four types of complaints, and present estimates of Equation 2 with the outcome being an indicator for each type of complaint in Columns (2) through (5) of Table 11. The first two, excessive force or verbal abuse (Column (2)) and improper search or arrest (Column (3)), constitute violations of an individual's constitutional rights during an officer's attempt to enforce the law. We also consider failure to provide service (FPS) complaints (Column (4)) and unbecoming conduct complaints (Column (5)). Analyzing different types of complaints enables us to distinguish between neglecting a civilian who desired help (for example, a potential victim of a crime), which would be an FPS complaint, and violations of a civilian's constitutional rights. Finally, complaints about unbecoming conduct cover actions like providing a false statement, being drunk and disorderly in public or on base, or insulting another officer.

We find no change in the use of force or verbal complaints following a former peer injury. However, complaints of a false arrest or improper search increase by nearly 11% in the week following a peer injury. This result is consistent with officers increasing the rate at which they violate a civilian's constitutional rights after a former peer is injured. We also find evidence that officers are much more likely to neglect civilians requesting help. Complaints citing a failure to provide service increase by 15.69% in the week following a peer injury. We do not find any evidence of additional complaints about unbecoming conduct.

5.7 HETEROGENEITY

To better understand how officers respond to peer injuries, we investigate heterogeneity based on officer tenure, the number of past events and suspect characteristics.

5.8 MODERATING EFFECTS OF OFFICER EXPERIENCE

We consider whether professional experience lessens the social influences in force-use. Guryan, Kroft and Notowidigdo (2009) suggests that professional experience attenuates social influences such as peer effects. Similarly, there is a large experimental literature suggesting that market experience contributes to individual rationality (List, 2003; List, Millimet et al., 2005; List, 2011; Tong et al., 2016). This suggests that as officers gain more experience on the job, they learn how to avoid responding emotionally to their former peers' injuries.

Table 12 displays the effect of former peer injuries as well as an interaction term between former peer injuries and the log of the officer's tenure in months since they started at the police academy for the outcomes considered so far. More experienced officers are less responsive to former peers' injuries in terms of using force, injuring suspects, or acting in a manner that causes a civilian to issue a complaint.[31] This finding is consistent with the evidence on experience and rationality and the general finding that social influence decreases with experience. With respect to policing, this finding is also consistent with those in Ta, Lande and Suss (2021) who use body camera data to show that a police officer's emotional reactivity is lower in more experienced police officers.

Next, we consider another form of experience: the number of times an officer has experienced an injury to a former peer. As mentioned in Section 4, officers experience multiple events over the time horizon in our sample. In general, the effect that repeated exposure has on an officer's responses is ambiguous. Officers may become increasingly agitated or risk-averse as they observe more peers being injured. Conversely, they may get used to learning about injuries to their former peers and respond less strongly. We investigate these effects in Table 13. Similar to the effect of tenure, we find

[31] Appendix Table A8 conducts the same analysis using tenure in levels. We find qualitatively similar results.

that repeated exposure to the injuries of former peers attenuates the effect of peer injuries.[32]

5.9 SUSPECT CHARACTERISTICS

Next, we investigate heterogeneity based on suspect race by estimating Equation 2 with the outcome being the force-use against a suspect of a particular race. We display the results in Table 14. We find that officers are significantly more likely to use force against Black suspects. There are no significant increases in the probability of using force against white or Hispanic suspects–though use of force against Hispanic suspects is imprecisely estimated.

Readers should use caution when interpreting these results. Roughly 81% of force uses and 80% of officer injuries result from interactions with Black suspects. As such, our results may be driven by the relatively small number of events observed for white and Hispanic suspects.

6. Mechanisms and Alternative Interpretations

Having established that police officers respond to peer injuries, we now attempt to understand what mechanisms might be driving this behavior. We begin by ruling out potentially confounding effects that would challenge our interpretation of the main finding. These include officers mimicking the force-use of their peers through traditional peer effects and officers increasing their effort after a peer is injured. Then, we investigate whether officers are responding to peer injuries because these injuries provide some information about their injury risk or because of transitory emotional responses.

[32] Appendix Table A9 conducts the same analysis using log tenure as the outcome. We find qualitatively similar results.

6.1 Confounding Effects

6.1.1 Officers Mimicking Peer Force-Use

First, we investigate whether officers are actually responding to their former peer's decision to use force rather than their former peer's injury. A large body of work shows that the actions of one's peers influence decision making (Brock and Durlauf, 2001). For example, Murphy (2019) finds that misconduct by soldiers in the U.S. Army tends to occur at similar times as the misconduct of peers. In our setting, a similar effect would be officers increasing their propensity to use force because a peer does so. This effect could potentially confound our results because in 94% of instances where officers were injured they also used force against the suspect (see Table 15).

We investigate this potential confounding effect using over 14,000 instances of force unaccompanied by an officer injury. We use these instances to investigate whether force-use mimicry is driving these results by estimating Equations 2 and 3 with the outcome being force-use and the treatment being an officer's former peer using force.

Column (1) of Table 16 shows that there is a strong correlation between an officer's use of force and the force-use of former peers that was unaccompanied by an officer injury. However, after we control for individual and district-week fixed effects, we find no significant relationship between the two facts. Column (5) of Table 16 and Figure 11 show that there is a small and insignificant effect of former peer's force-use on an officer's force-use. Therefore, we can conclude that our results are not driven by officers mimicking the use of force by their former peers.

6.1.2 Officers Increasing Effort

Next, we investigate whether officers increase their time working or effort following an injury to a former peer. A potential issue is that officers have to work more hours after a peer injury because the peer has been removed from the pool of eligible workers. This is unlikely for two reasons. First, we are using events that occur to former peers. That means that the injured officers work in a separate police district. Second, there is no reason to expect that cohort-mates of the injured officer working in different districts should be differentially affected by such events relative to other officers in different districts. Alternatively, it may be that officers reduce their effort on the job for fear of being injured.

To investigate potential changes in police effort and tike working, we use arrests as a measure of officer effort, following Mas (2006) and Ba and Rivera (2019). Column (1) in Table 17 shows the impact of former peer injuries on the probability of arresting a suspect for any reason in the following week. Figure 12 displays the dynamics. Overall, we find that there is a small positive impact on officers' effort as measured by arrests.[33].

Columns (2) through (5) in Table 17 shows the impact of former peer injuries on different types of arrests. The arrest types correspond to the crime types: violent, property, and non-index crimes. We find that non-index crime arrests increase by 1.8% in the week following a peer injury. In contrast, property and violent crime arrests remain unaffected, indicating officers are unlikely to be working more. Furthermore, officers appear to be increasing their discretionary (non-index) arrests, suggesting that they are not attempt-

[33] In order to avoid issues with mismeasuring the week of the arrest due to the lack of precise arrest dates pre-2010, we redo the analysis on observations from 2010-2016 in Table A13, and the results are almost identical

ing to avoid potentially dangerous situations. These results suggest that officers do not materially decrease their effort after they experience a peer injury.

6.2 Social Learning and Emotional Responses

Now that we have ruled out officers increasing their time spent working, decreasing their effort on the job, or mimicking their peer's force-use, we seek to determine why officers respond to peer injuries. We consider whether social learning (Banerjee, 1992; Bikhchandani, Hirshleifer and Welch, 1992) or emotional responses (Rick and Loewenstein, 2008; Kőszegi, 2006) are more likely to drive the results.

Social learning will drive the results when officers are more likely to learn about their former peers' injuries and use this information in their future decisions to use force. Injuries to former peers may act as a private signal of the underlying injury risk. The signal could cause officers to update their beliefs about the probability that a non-compliant civilian will injure them, increasing their propensity to use force.

On the other hand, previous literature has also documented that negative emotional states can influence an individual's propensity to engage in violence (Card and Dahl, 2011; Munyo and Rossi, 2015; Eren and Mocan, 2018). Several laboratory experiments also show that exposure to violence can affect time and risk preferences. Loewenstein (1996) documented that preferences can be malleable and can be temporarily affected by emotional states. For example, traumatic events and natural disasters can impact risk-preferences (Cameron and Shah, 2015; Tanaka, Camerer and Nguyen, 2010; Hanaoka, Shigeoka and Watanabe, 2018). Alternatively, Hjort (2014) finds that animus discrimination can increase in response to ethnic conflict, and Rohlfs (2010) finds that exposure to violence can make individuals more violent.

Two main predictions separate these mechanisms. First, if social learning is driving the results, officers would have a lower chance of experiencing an injury themselves in the week following a peer injury as they have better information about their true injury risk while on duty. We investigate this by estimating Equations 2 and 3, with the outcome being an indicator representing the officer's injury status. We report these results in Figure 13 and Table 18. We find that injury risk falls by 7.26% in the week after a former peer is injured (Column (5)). However, the results are not statistically significant.

Second, the two mechanisms will also differentially affect the resulting dynamics. Under social learning, former peers learn about the treatment effects more quickly than those who are not former peers. In that case, the treatment group should have a constant effect while the control group's propensity to use force should increase to match that of the treatment group. In contrast, under an emotional response mechanism, the effects should dissipate because officers stop responding within a few weeks. Loewenstein and O'Donoghue (2007) point out that the temporal proximity to the event greatly impacts emotional responses. Indeed, Card and Dahl (2011) and Munyo and Rossi (2015) both find that the emotional responses to sports losses are concentrated in a narrow time window after the game.

We display the effects separately by those experiencing an injury to a former peer (treatment) and those not experiencing an injury to a former peer (control) in Figure 14. In line with the emotional response mechanism, the control officers' propensity to use force does not increase. In contrast, the treatment officer's propensity to use force increases in the weeks immediately after the event and then return to their baseline level of force-use soon thereafter. Although we cannot fully rule out a role for social learning, these results strengthen the case for the emotional response interpretation.

7. CONCLUSION

This article shows that on-duty injuries to police officers can have spillovers onto how other officers interact with civilians. Following a force-related injury to a police academy classmate working in a different district, officers increase their propensity to use force by 7% and increase their propensity to injure a suspect by 10% in the following week. Furthermore, these effects double in magnitude when the injured peer is of the same race. Given that we focus on peers acquired through the police academy and do not consider effects from peers currently in the same unit, these results likely underestimate the total effect of peer injuries.

We do not find evidence that these effects are driven by officers mimicking their peers' force-use, changing their effort, or behaving differently due to social learning. Rather, we show that the increase in force-use is concentrated in a narrow time window around the event and that the effects fade with officer tenure. Collectively, the results suggests that the effects are driven by an emotional response with officers becoming more aggressive during interactions.

The existence of spillovers in the police force-use resulting from on-the-job injuries has important implications for policies meant to reduce improper use of force. Policies that have been shown to decrease officer injuries, such as increased patrol sizes (Kirchmaier et al., 2021), may have additional benefits for civilian and officer safety by avoiding spillovers. Other policies such as police militarization, which can increase officer injuries may have negative ripple effects by leading to more dangerous encounters for civilians (Masera, 2021). Policymakers should take these externalities into account when determining the optimal way to reduce improper use of force.

Focusing on interventions that reduce injury risk may reduce the threat to officers and will have the added benefit of reducing their propensity to use force. Any policy meant to reduce force-use that increases the risk to officers may have limited effects. Our results suggest that providing counseling or other support services to traumatized officers after a peer is injured (or a different incident occurs) may reduce the instances of force ((Owens et al., 2018)).[34] Moreover, future research should attempt to identify interventions that can help prevent officer injuries without increasing the risk of police violence faced by civilians.

[34]To the best of our knowledge, there is very little research on the impact of support services on officers when they experience trauma. However, (Owens et al., 2018) show promising results indicating that low-intensity supervisory programs can lead officers to resolve incidents without making any arrest or using force, hence without increasing the risk of injuries for both the officer or the civilian.

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8. Tables

Table 1: Characteristics of All Officers who Start at the Police Academy

	count	mean	sd	min	max
Officer Male	4429	0.77	0.42	0	1
Officer Black	4429	0.17	0.37	0	1
Officer White	4429	0.49	0.50	0	1
Officer Hispanic	4429	0.34	0.47	0	1
Officer Age	4429	28.8	4.39	21	42

Note: This table reports descriptive statistics for all officers who enter the police academy during our sample period. Age at test is proxied by the officer's age at the most recent police exam.

Table 2: Characteristics of Officers who Enter Geographic Police Units

	count	mean	sd	min	max
Officer Male	3461	0.78	0.41	0	1
Officer Black	3461	0.17	0.37	0	1
Officer White	3461	0.49	0.50	0	1
Officer Hispanic	3461	0.34	0.47	0	1
Officer Age	3461	28.8	4.34	21	42

Note: Age is measured at the age of taking the entrance exam. Age at test is proxied by the officer's age at the most recent police exam.

Table 3: Frequency of Events and Outcomes

	Weeks	Mean	Standard Deviation	10th Percentile	50th Percentile	90th Percentile
Injuries:						
Officer Injured	675	0.003	0.002	0.000	0.002	0.005
Former Peer Injured	675	0.105	0.074	0.004	0.094	0.214
Cohort Member Injured	675	0.112	0.078	0.005	0.100	0.227
Any Officer Injured	675	0.904	0.293	0.957	1.000	1.000
Force Use:						
Any Force Use	675	0.018	0.007	0.010	0.018	0.027
Control	675	0.011	0.005	0.005	0.010	0.017
Without Weapon	675	0.015	0.006	0.008	0.014	0.022
Nonlethal	675	0.001	0.002	0.000	0.001	0.003
Mitigation	675	0.019	0.007	0.011	0.019	0.028
Baton	675	0.001	0.001	0.000	0.000	0.002
Taser	675	0.001	0.002	0.000	0.000	0.003
Firearm	675	0.000	0.001	0.000	0.000	0.001
Other	675	0.001	0.001	0.000	0.001	0.003
Injured Suspect	675	0.006	0.003	0.002	0.005	0.010
Arrests:						
Any Crime	675	0.527	0.268	0.313	0.420	1.000
Municipal Code	675	0.034	0.028	0.015	0.023	0.076
Traffic	675	0.053	0.026	0.032	0.046	0.087
Warrant	675	0.113	0.061	0.066	0.093	0.221
Drug Crime	675	0.173	0.098	0.084	0.146	0.346
Property Crime	675	0.096	0.059	0.047	0.073	0.198
Violent Crime	675	0.135	0.067	0.073	0.112	0.244
Other	675	0.177	0.103	0.093	0.140	0.364
Complaints:						
All Complaints	675	0.016	0.008	0.007	0.015	0.026
Force and Verbal	675	0.004	0.004	0.000	0.004	0.010
Arrest and Search	675	0.006	0.004	0.000	0.005	0.010
Failure to Provide Service	675	0.003	0.003	0.000	0.003	0.006
Unbecoming Conduct	675	0.000	0.001	0.000	0.000	0.001

Note: This table reports descriptive statistics for each of the weeks in the data set. Since officers are joining throughout the sample period, the composition of officers differs across weeks. The mean value represents the probability that each event or outcome occurred at least once in the sample week.

Table 4: Covariance of Events/Outcomes and Characteristics

	(1) Officer Injured	(2) Force	(3) Injures Suspect	(4) Arrest	(5) Complaint
Any Officer Injured in Previous Week	0.00014	0.00235	0.00065	-0.08277	-0.00124
	(0.00028)	(0.00074)	(0.00041)	(0.00519)	(0.00075)
Member of Same Unit Injured in Previous Week	0.00009	0.00114	0.00018	-0.00005	-0.00050
	(0.00013)	(0.00038)	(0.00021)	(0.00212)	(0.00032)
Member of Same Cohort Officer Injured in Previous Week	-0.00013	0.00165	0.00047	0.00519	0.00094
	(0.00016)	(0.00047)	(0.00025)	(0.00241)	(0.00040)
Officer is Female	-0.00060	-0.01018	-0.00374	-0.11010	-0.00422
	(0.00013)	(0.00054)	(0.00020)	(0.00877)	(0.00050)
Officer is Black	-0.00113	-0.00522	-0.00145	-0.06434	-0.00032
	(0.00018)	(0.00080)	(0.00031)	(0.01101)	(0.00068)
Officer is Hispanic or Other	-0.00037	-0.00245	-0.00064	-0.00435	-0.00085
	(0.00016)	(0.00068)	(0.00028)	(0.00866)	(0.00052)
Officer Age at Test	-0.00008	-0.00055	-0.00019	-0.00811	-0.00040
	(0.00001)	(0.00006)	(0.00002)	(0.00087)	(0.00005)
Officer Tenure	-0.00001	-0.00008	-0.00003	-0.00154	-0.00005
	(0.00000)	(0.00001)	(0.00000)	(0.00010)	(0.00001)
Portion of Cohort that is Male	-0.00102	0.00062	-0.00053	-0.16737	-0.00939
	(0.00089)	(0.00378)	(0.00154)	(0.05524)	(0.00336)
Portion of Cohort that is Black	-0.00002	0.00368	-0.00082	0.01120	0.00132
	(0.00094)	(0.00405)	(0.00156)	(0.05385)	(0.00323)
Portion of Cohort that is Hispanic or Other	-0.00081	0.00072	-0.00003	-0.08618	-0.00468
	(0.00080)	(0.00356)	(0.00139)	(0.04873)	(0.00287)
Average Age of Cohort	0.00006	-0.00023	0.00001	0.00856	0.00054
	(0.00005)	(0.00023)	(0.00009)	(0.00306)	(0.00017)
Portion of Unit that is Male	0.00407	0.04753	0.01375	0.40126	0.01330
	(0.00180)	(0.00724)	(0.00285)	(0.10251)	(0.00536)
Portion of Unit that is Black	0.00215	0.01320	0.00423	0.16204	0.02058
	(0.00078)	(0.00303)	(0.00127)	(0.03798)	(0.00252)
Portion of Unit that is Hispanic or Other	0.00082	-0.00721	0.00018	-0.01956	0.01158
	(0.00105)	(0.00458)	(0.00183)	(0.05875)	(0.00348)
Average Age of Unit	-0.00007	-0.00032	-0.00013	-0.00223	-0.00141
	(0.00007)	(0.00032)	(0.00013)	(0.00464)	(0.00024)
Constant	0.00435	0.02371	0.00848	0.55632	0.05775
	(0.00361)	(0.01544)	(0.00617)	(0.22962)	(0.01165)
R-squared	0.000	0.004	0.002	0.057	0.002
Observations	986,111	986,111	986,111	953,567	986,111

Note: This table displays regression coefficient estimates from regressions of various outcomes on officer injuries and characteristics at the officer-week level. Lagged any officer, unit-member, and cohort-member injured are indicators representing whether any other officer in the police force, same unit, or same academy cohort were injured in the previous week. Characteristics of cohorts and units are calculated as leave-out means. Standard errors are clustered on the individual level.

Table 5: Testing the Random Assignment of Police Entrance Lotteries

	(1) Officer Male	(2) Officer Black	(3) Officer White	(4) Officer Hispanic	(5) Office Age
Class Cohort Leave-out-mean	0.22	0.12	0.04	-0.05	0.52
	(0.12)	(0.11)	(0.13)	(0.14)	(0.16)
Test Group Leave-out-mean	-110.70	-150.89	-120.92	-111.25	-96.25
	(54.49)	(58.86)	(56.94)	(55.85)	(52.23)
Constant	86.79	25.60	60.34	37.76	2,610.27
	(42.50)	(9.95)	(28.23)	(18.78)	(1,409.41)
R-squared	0.346	0.423	0.351	0.329	0.342
Observations	3,468	3,468	3,468	3,468	3,468

Note: Table includes results from estimating Equation 1 with various predetermined officer-level characteristics. Sample includes every officer who started at the police academy between January 2002 and December 2013 regardless of whether or not they work in a geographic unit after graduation. Officer age is measured at the age of taking the entrance exam. Standard errors are in parenthesis and are clustered by the test cohort. We assign officers to a test academy cohort based on the last test that occurred before they began at the police academy. However, we do not observe the officer's actual test date.

Table 6: Effect of Injuries to Former Peers on the Propensity to use Force

	(1) Force	(2) Force	(3) Force	(4) Force	(5) Force	(6) Force
Former peer in previous week	0.00419	0.00321	0.00178	0.00123	0.00123	0.05805
	(0.00078)	(0.00076)	(0.00061)	(0.00058)	(0.00054)	(0.02469)
Constant	0.01720	0.01732	0.01748	0.01754	0.01754	-3.01291
	(0.00057)	(0.00057)	(0.00044)	(0.00036)	(0.00006)	(0.00337)
Model	OLS	OLS	OLS	OLS	OLS	Poisson
Percent Increase	24.370	18.630	10.370	7.170	7.160	5.810
Unit-Week Fixed Effects	NO	YES	YES	YES	YES	YES
Number of Former Peers	NO	NO	YES	YES	NO	NO
Test Period Fixed Effects	NO	NO	NO	YES	NO	NO
Individual Fixed Effects	NO	NO	NO	NO	YES	YES
Pre-trend Test	0.000	0.000	0.177	0.793	0.804	0.915
R-squared	0.000	0.022	0.023	0.024	0.040	
Observations	986,111	986,088	986,088	986,088	986,088	607,688

Note: Column (1) displays estimates from a linear regression of an indicator for any force used by the officer on the first lag of injuries to former peers. Column (2) controls for unit-week fixed effects. Column (3) controls for unit-week and number of former peer fixed effects. Column (4) controls for unit-week, number of former peers, and estimated test period fixed effects. Column (5) estimates Equation 2, controlling for individual and unit-week fixed effects. Column (6) estimates Equation 2 using Poisson maximum likelihood estimation. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Figure 6 displays all percent changes from Column (5) in this regression testing for differences in pre-trends. Column (1) in Appendix Table A4 displays all of the coefficients on lags and leads for Column (6) in this table.

Table 7: Effect of Former Peer Injuries on Suspect Injuries

	(1) Injure Suspect	(2) Injure Suspect	(3) Injure Suspect	(4) Injure Suspect	(5) Injure Suspect	(6) Injure Suspect
Former peer in previous week	0.00140	0.00122	0.00074	0.00054	0.00053	0.09298
	(0.00035)	(0.00036)	(0.00032)	(0.00031)	(0.00030)	(0.04382)
Constant	0.00528	0.00530	0.00535	0.00537	0.00537	-3.21079
	(0.00022)	(0.00021)	(0.00018)	(0.00015)	(0.00003)	(0.00607)
Model	OLS	OLS	OLS	OLS	OLS	Poisson
Percent Increase	26.490	23.050	14.100	10.240	10.060	9.300
Test Period Fixed Effects	NO	YES	YES	YES	YES	YES
Unit-Week Fixed Effects	NO	NO	NO	YES	NO	NO
Number of Former Peers	NO	NO	NO	NO	YES	YES
Individual Fixed Effects	NO	NO	YES	YES	NO	NO
Pre-trend Test	0.000	0.002	0.032	0.086	0.085	0.097
R-squared	0.000	0.021	0.022	0.022	0.031	
Observations	986,111	986,088	986,088	986,088	986,088	233,326

Note: Column (1) displays estimates from a linear regression of an indicator for a suspect injury on the first lag of injuries to former peers. Column (2) controls for unit-week fixed effects. Column (3) controls for unit-week and number of former peer fixed effects. Column (4) controls for unit-week, number of former peers, and estimated test period fixed effects. Column (5) estimates Equation 2, controlling for individual and unit-week fixed effects. Column (6) estimates Equation 2 using Poisson maximum likelihood estimation. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Figure 7 displays all percent changes from Column (5) in this regression testing for differences in pre-trends. Column (2) in Appendix Table A4 displays all of the coefficients on lags and leads for Column (6) in this table.

Table 8: Heterogeneous Effects by Type of Force

	(1) Control	(2) No Weapon	(3) Non-Lethal	(4) Baton	(5) Taser	(6) Firearm	(7) Other
Former peer in previous week	0.00072	0.00095	-0.00002	-0.00004	0.00009	0.00012	0.00007
	(0.00035)	(0.00048)	(0.00008)	(0.00007)	(0.00013)	(0.00006)	(0.00015)
Constant	0.01030	0.01438	0.00086	0.00050	0.00167	0.00027	0.00108
	(0.00004)	(0.00005)	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00002)
Model	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Percent Increase	8.108	7.627	-1.914	-8.818	6.907	46.104	0.007
Unit-Week Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Pre-trend Test	0.684	0.742	0.010	0.407	0.495	0.923	0.545
R-squared	0.034	0.039	0.029	0.022	0.024	0.022	0.023
Observations	986,088	986,088	986,088	986,088	986,088	986,088	986,088

Note: Columns (1) through (7) display coefficients from estimates of Equation 2 where the outcome variable is an indicator representing whether the officer used a specific type of force. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Appendix Table A5 displays all coefficients from this regression testing for differences in pre-trends.

Table 9: Effect of Injuries to Former Peers of the Same Race

	(1) Force	(2) Force	(3) Force	(4) Force	(5) Force	(6) Force
Same-race former peer injured in previous week	0.00723	0.00616	0.00469	0.00416	0.00286	0.12505
	(0.00106)	(0.00106)	(0.00092)	(0.00091)	(0.00089)	(0.03579)
Constant	0.01734	0.01739	0.01746	0.01749	0.01755	-3.01792
	(0.00058)	(0.00057)	(0.00043)	(0.00036)	(0.00004)	(0.00230)
Model	OLS	OLS	OLS	OLS	OLS	Poisson
Percent Increase	41.711	35.541	27.026	23.960	16.487	12.505
Unit-Week Fixed Effects	NO	YES	YES	YES	YES	YES
Number of Former Peers	NO	NO	YES	YES	NO	NO
Test Period Fixed Effects	NO	NO	NO	YES	NO	NO
Individual Fixed Effects	NO	NO	NO	NO	YES	YES
Pre-trend Test	0.000	0.000	0.000	0.003	0.363	0.587
R-squared	0.000	0.022	0.023	0.024	0.040	
Observations	986,111	986,088	986,088	986,088	986,088	607,808

Note: Column (1) displays estimates from a linear regression of an indicator for any force used by the officer on the first lag of injuries to same-race former peers. Column (2) controls for unit-week fixed effects. Column (3) controls for unit-week and number of former peer fixed effects. Column (4) controls for unit-week, number of former peers, and estimated test period fixed effects. Column (5) estimates Equation 2, controlling for individual and unit-week fixed effects. Column (6) estimates Equation 2 using Poisson maximum likelihood estimation. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Figure 8 displays all percent changes from Column (5) in this regression testing for differences in pre-trends. Column (6) in Appendix Table A4 displays all of the coefficients on lags and leads for Column (6) in this table.

Table 10: Effect of Low-Resistance Injuries to Former Peers on Force-Use

	(1) Force	(2) Force	(3) Force	(4) Force	(5) Force	(6) Force
Former peer in previous week	0.00335	0.00238	0.00090	0.00048	0.00037	0.01613
	(0.00076)	(0.00090)	(0.00068)	(0.00061)	(0.00056)	(0.02862)
Constant	0.01748	0.01753	0.01762	0.01765	0.01766	-3.00629
	(0.00059)	(0.00058)	(0.00044)	(0.00035)	(0.00003)	(0.00204)
Model	OLS	OLS	OLS	OLS	OLS	Poisson
Percent Increase	19.176	13.641	5.124	2.741	2.096	1.613
Unit-Week Fixed Effects	NO	YES	YES	YES	YES	YES
Number of Former Peers	NO	NO	YES	YES	NO	NO
Test Period Fixed Effects	NO	NO	NO	YES	NO	NO
Individual Fixed Effects	NO	NO	NO	NO	YES	YES
Pre-trend Test	0.000	0.002	0.034	0.068	0.079	0.101
R-squared	0.000	0.022	0.023	0.024	0.040	
Observations	986,111	986,088	986,088	986,088	986,088	607,688

Note: Column (1) displays estimates from a linear regression of an indicator for any force used by the officer on the first lag of injuries to former peers. In this table, ”injury” refers to injuries to officers for which the officer reported that the suspect did not use physical resistance. Column (2) controls for unit-week fixed effects. Column (3) controls for unit-week and number of former peer fixed effects. Column (4) controls for unit-week, number of former peers, and estimated test period fixed effects. Column (5) estimates Equation 2, controlling for individual and unit-week fixed effects. Column (6) estimates Equation 2 using Poisson maximum likelihood estimation. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Figure 6 displays all percent changes from Column (5) in this regression testing for differences in pre-trends. Column (1) in Appendix Table A6 displays all of the coefficients on lags and leads for Column (6) in this table.

Table 11: Effect of Former Peer Injuries on Complaints Against Officers

	(1) All Complaints	(2) Force and Verbal	(3) Arrest and Search	(4) Failure to Provide Service	(5) Unbecoming Conduct
Former peer in previous week	0.00083	-0.00013	0.00048	0.00036	0.00001
	(0.00039)	(0.00020)	(0.00020)	(0.00018)	(0.00006)
Constant	0.01309	0.00339	0.00496	0.00250	0.00026
	(0.00004)	(0.00002)	(0.00002)	(0.00002)	(0.00001)
Percent Increase	6.838	-4.340	10.468	15.541	0.001
Unit-Week Fixed Effects	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES
Pre-trend Test	0.412	0.516	0.117	0.468	0.550
R-squared	0.039	0.033	0.037	0.027	0.027
Observations	986,088	986,088	986,088	986,088	986,088

Note: Columns (1) through (5) display coefficients from estimates of Equation 2 where the outcome variable is an indicator representing types of complaints against the officer. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level ($G = 81$). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Appendix Table A7 displays all coefficients from this regression testing for differences in pre-trends.

Table 12: Heterogeneous Effects by Log Tenure

	(1) Force	(2) Injure Suspect	(3) Arrest	(4) Officer Injured	(5) Complaint
Former peer in previous week × Log Tenure (months)	-0.00262	-0.00127	-0.00038	-0.00001	-0.00119
	(0.00112)	(0.00065)	(0.00337)	(0.00033)	(0.00069)
Former peer in previous week	0.01149	0.00551	0.00532	-0.00013	0.00550
	(0.00460)	(0.00268)	(0.01377)	(0.00133)	(0.00270)
Constant	0.01753	0.00537	0.37589	0.00236	0.01308
	(0.00006)	(0.00003)	(0.00025)	(0.00002)	(0.00004)
Model	OLS	OLS	OLS	OLS	OLS
Unit-Week Fixed Effects	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES
Pre-trend Test	NO	NO	NO	NO	NO
R-squared	0.742	0.159	0.493	0.070	0.491
Observations	0.040	0.031	0.262	0.026	0.039
N	986,088	986,088	953,262	986,088	986,088

Note: Columns (1) through (5) display coefficients from estimates of Equation 2 with various indicators and an interaction term between a lagged injury to a former peer and the officer tenure. Log Officer tenure is a continuous variable representing the log of the number of months since the officer started at the police academy. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table 13: Heterogeneous Effects by Log Number of Past Events

	(1) Force	(2) Injure Suspect	(3) Arrest	(4) Officer Injured	(5) Complaint
Former peer in previous week × Number of Previous (months)	-0.00094	-0.00031	0.00425	0.00002	-0.00002
	(0.00051)	(0.00032)	(0.00218)	(0.00017)	(0.00040)
Former peer in previous week	0.00479	0.00167	-0.01478	-0.00028	0.00081
	(0.00213)	(0.00136)	(0.00911)	(0.00070)	(0.00154)
Constant	0.01739	0.00533	0.37374	0.00236	0.01298
	(0.00006)	(0.00004)	(0.00028)	(0.00002)	(0.00004)
Model	OLS	OLS	OLS	OLS	OLS
Unit-Week Fixed Effects	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES
Pre-trend Test	NO	NO	NO	NO	NO
R-squared	0.742	0.159	0.493	0.070	0.491
Observations	0.041	0.031	0.264	0.027	0.040
N	947,579	947,579	921,341	947,579	947,579

Note: Columns (1) through (5) display coefficients from estimates of Equation 2 with various indicators and an interaction term between a lagged injury to a former peer and the logged number of previous events + 1. Number of previous events is a continuous variable representing the number of times the officer has experienced an injury to a former peer. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table 14: Heterogeneous Effects by Suspect Characteristics

	(1) White Suspect	(2) Minority Suspect	(3) Male Suspect	(4) Female Suspect
Former peer in previous week	0.00008	0.00114	0.00115	0.00009
	(0.00012)	(0.00049)	(0.00056)	(0.00013)
Constant	0.00112	0.01619	0.01509	0.00251
	(0.00001)	(0.00006)	(0.00006)	(0.00001)
Model	OLS	OLS	OLS	OLS
Percent Increase	6.883	7.200	7.782	0.009
Unit-Week Fixed Effects	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES
Pre-trend Test	0.697	0.835	0.817	0.985
R-squared	0.036	0.039	0.039	0.026
Observations	986,088	986,088	986,088	986,088

Note: Columns (1) through (3) display coefficients from estimates of Equation 2 where the outcome variable is an indicator representing whether the officer used a specific type of force against a suspect of a given race. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Appendix Table A10 displays all coefficients from this regression testing for differences in pre-trends.

Table 15: Force-Use and Injuries

	Did not use Force	Used Force	Total
Not Injured	972,306	15,291	987,597
Injured	125	2,186	2,311
Total	972,431	17,477	989,908

Note: This table displays the frequency of force-use and injuries for every officer-week observation in our sample.

Table 16: Effect of Former Peer Force-Use on Officer Force-Use

	(1) Force	(2) Force	(3) Force	(4) Force	(5) Force	(6) Force
Former peer in previous week	0.00501	0.00365	0.00167	0.00053	0.00045	0.02629
	(0.00061)	(0.00061)	(0.00037)	(0.00032)	(0.00033)	(0.01754)
Constant	0.01520	0.01587	0.01685	0.01742	0.01745	-3.01996
	(0.00052)	(0.00055)	(0.00043)	(0.00038)	(0.00016)	(0.00992)
Model	OLS	OLS	OLS	OLS	OLS	Poisson
Percent Increase	32.948	24.041	10.999	3.483	2.980	2.629
Unit-Week Fixed Effects	NO	YES	YES	YES	YES	YES
Number of Former Peers	NO	NO	YES	YES	NO	NO
Test Period Fixed Effects	NO	NO	NO	YES	NO	NO
Individual Fixed Effects	NO	NO	NO	NO	YES	YES
Pre-trend Test	0.000	0.000	0.000	0.643	0.657	0.733
R-squared	0.000	0.022	0.023	0.024	0.040	
Observations	986,111	986,088	986,088	986,088	986,088	607,688

Note: Columns (1) through (7) display coefficients from estimates of Equation 2 where the outcome variable is an indicator representing whether the officer used force and the event is whether the officer's former peer used force but was not injured in the previous week. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Figure 11 displays all percent changes from Column (5) in this regression testing for differences in pre-trends. Column (5) in Appendix Table A11 displays all of the coefficients on lags and leads for Columns (5) and (6) in this table.

Table 17: Effect of Former Peer Injuries on Officer Arrests

	(1) Any Arrest	(2) Non-Index Crime	(3) Property Crime	(4) Violent Crime
Former peer in previous week	0.00384	0.00397	0.00096	-0.00007
	(0.00218)	(0.00171)	(0.00099)	(0.00125)
Constant	0.37589	0.21881	0.06364	0.09899
	(0.00025)	(0.00020)	(0.00011)	(0.00014)
Model	OLS	OLS	OLS	OLS
Percent Increase	1.034	1.835	1.532	-0.007
Unit-Week Fixed Effects	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES
Pre-trend Test	0.423	0.675	0.052	0.106
R-squared	0.262	0.250	0.077	0.078
Observations	953,262	953,262	953,262	953,262

Note: Columns (1) through (8) display coefficients from estimates of Equation 2 where the outcome variable is an indicator representing arrests for various types of crime. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Appendix Table A12 displays all coefficients from this regression testing for differences in pre-trends.

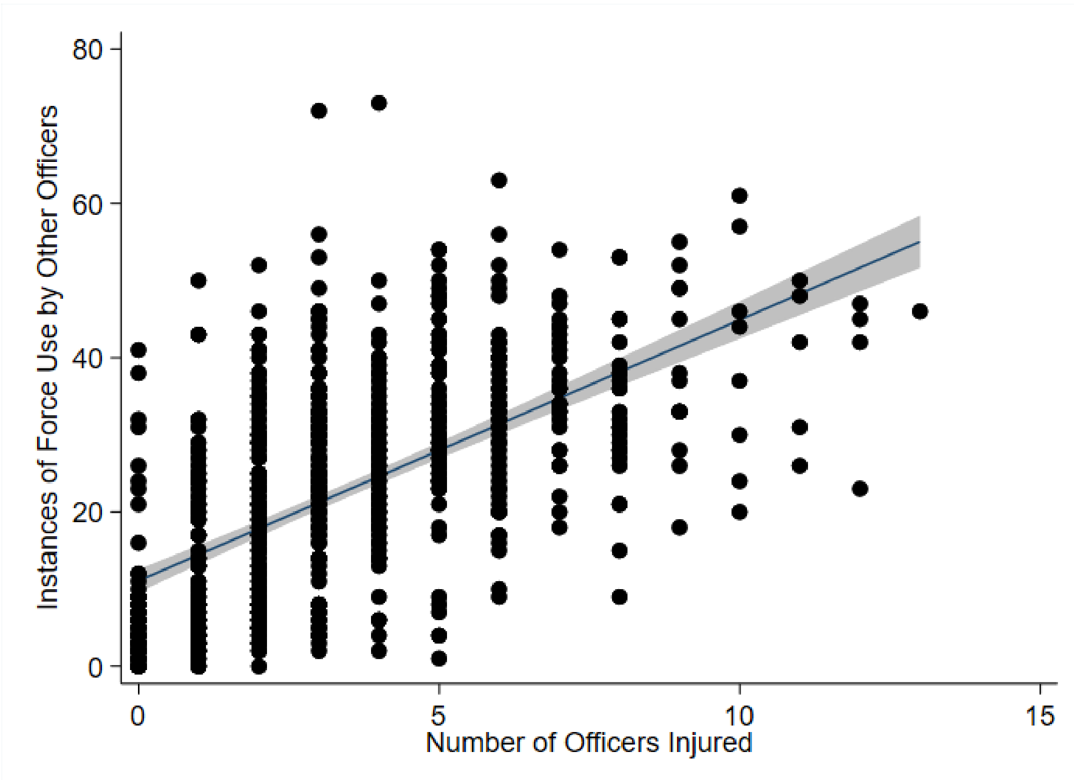
Table 18: Effect of Former Peer Injuries on Officer Injuries

	(1) Injured	(2) Injured	(3) Injured	(4) Injured	(5) Injured	(6) Injured
Former peer in previous week	0.00021	0.00004	-0.00014	-0.00019	-0.00017	-0.07594
	(0.00019)	(0.00018)	(0.00017)	(0.00017)	(0.00016)	(0.07055)
Constant	0.00231	0.00233	0.00235	0.00236	0.00236	-3.10241
	(0.00007)	(0.00008)	(0.00007)	(0.00006)	(0.00002)	(0.00859)
Model	OLS	OLS	OLS	OLS	OLS	Poisson
Percent Increase	8.860	1.820	-6.130	-8.370	-7.260	-7.590
Unit-Week Fixed Effects	NO	YES	YES	YES	YES	YES
Number of Former Peers	NO	NO	YES	YES	NO	NO
Test Period Fixed Effects	NO	NO	NO	YES	NO	NO
Individual Fixed Effects	NO	NO	NO	NO	YES	YES
Pre-trend Test	0.000	0.003	0.054	0.108	0.085	0.085
R-squared	0.000	0.020	0.020	0.020	0.026	
Observations	986,111	986,088	986,088	986,088	986,088	88,582

Note: Columns (1) through (7) display coefficients from estimates of Equation 2 where the outcome variable is an indicator representing whether the officer experienced an injury. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. Column (5) in Appendix Table A4 displays all of the coefficients on lags and leads for Column (6) in this table.

9. Figures

Figure 1: Correlation Between Officer Injuries and Force-Use by Others



Note: This graph displays the relationship between the number of officers injured in a given week and the number of uninjured officers who use force in that same week. It uses the full sample of all officers included in Tactical Response Reports from 2004 to 2016. The blue line represents the regression line of force-use in a given week on the number of other officers who are injured in that week. Standard error bands are presented around the line.

Figure 2: New Officers Joining

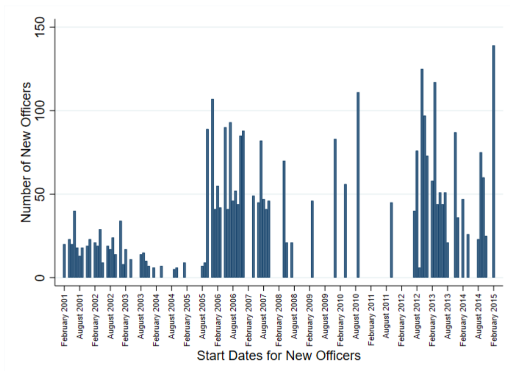


Figure 3: Cohort Size

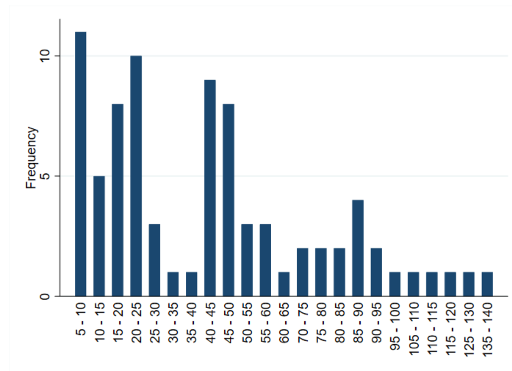


Figure 4: Unit Sizes

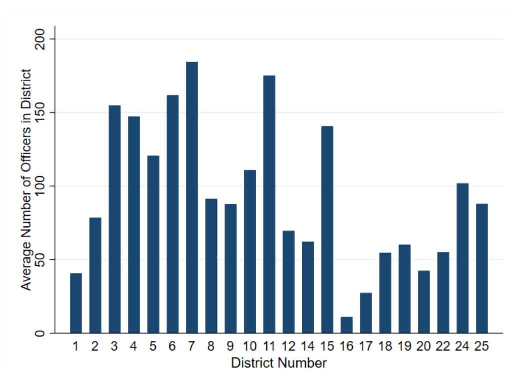


Figure 5: Number of Former Peers

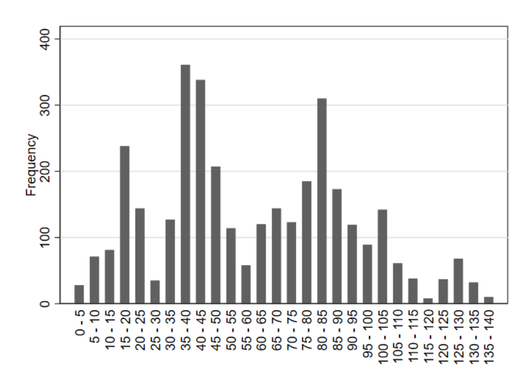
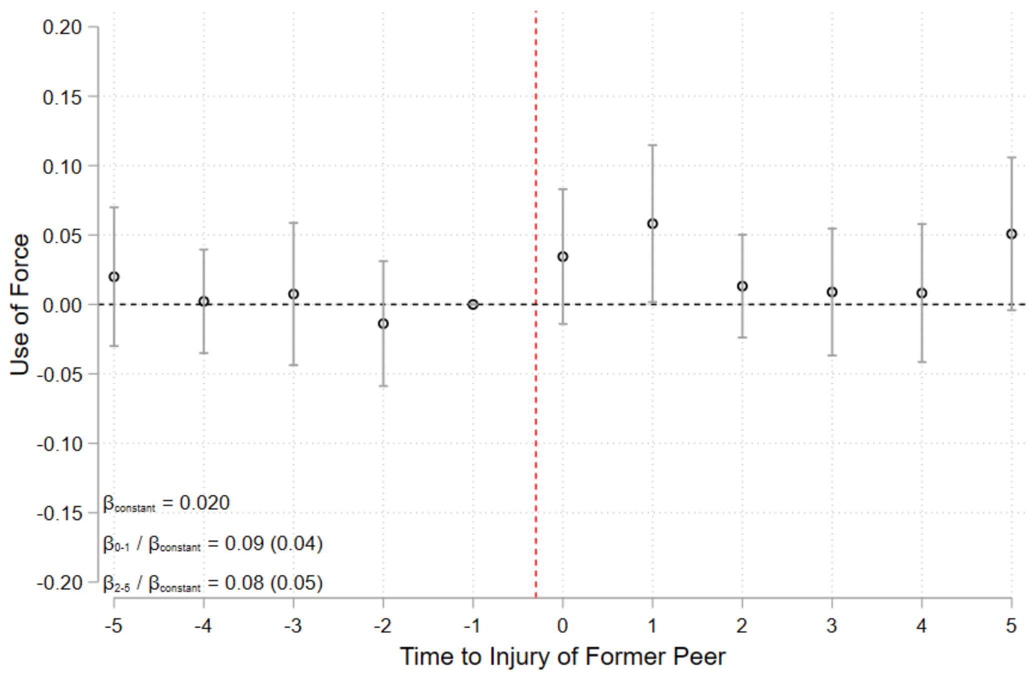
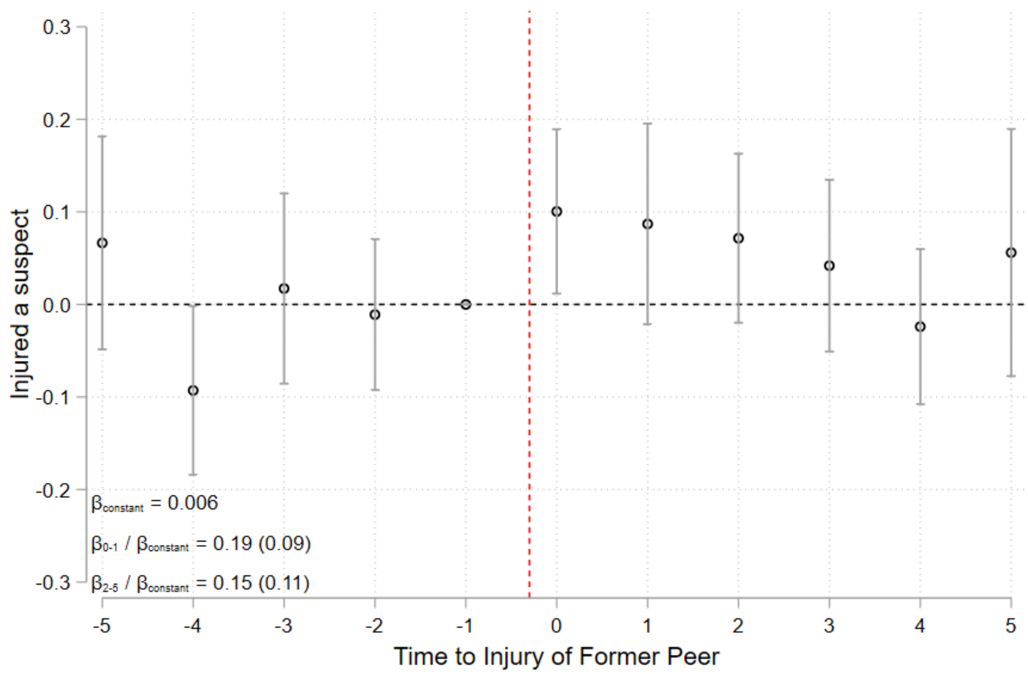


Figure 6: The Effect of Former Peer Injuries on Police Use of Force



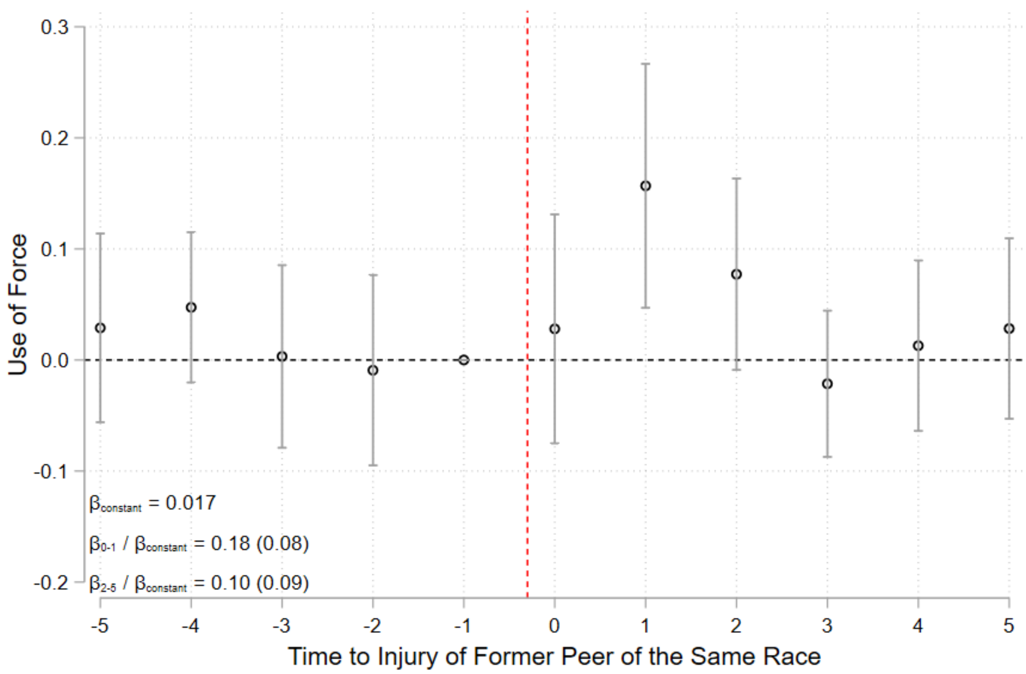
Note: The graph shows coefficient estimates using Equation 3 divided by the baseline rate of force-use and 95% confidence intervals. The baseline rate of force is calculated as the constant term from a regression of force on treatment lags and leads without fixed effects. Standard errors clustered by academy cohort (G = 81). The regression includes individual and unit-week fixed effects. Treatment is an injury of a former peer. Red vertical line represents the injury week.

Figure 7: The Effect of Former Peer Injuries on Suspect Injuries



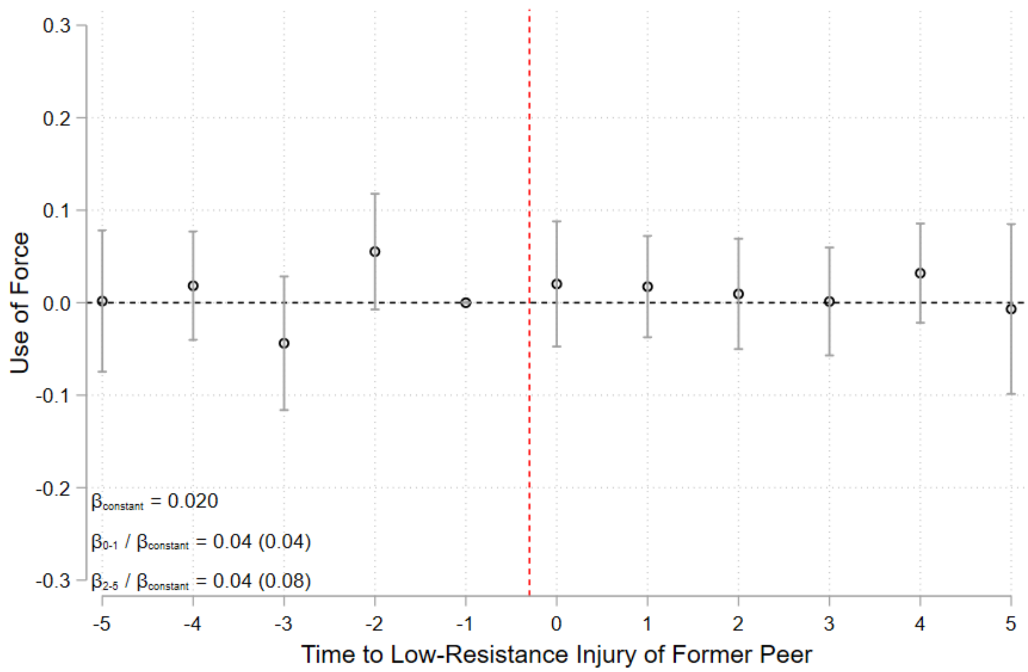
Note: The graph shows coefficient estimates using Equation 3 divided by the baseline rate of suspect injuries and 95% confidence intervals. The baseline rate of suspect injuries is calculated as the constant term from a regression of suspect injuries on treatment lags and leads without fixed effects. Standard errors clustered by academy cohort (G = 81). The regression includes individual and unit-week fixed effects. Treatment is an injury of a former peer. Red vertical line represents the injury week.

Figure 8: The Effect of Same Race Former Peer Injuries on Force-Use



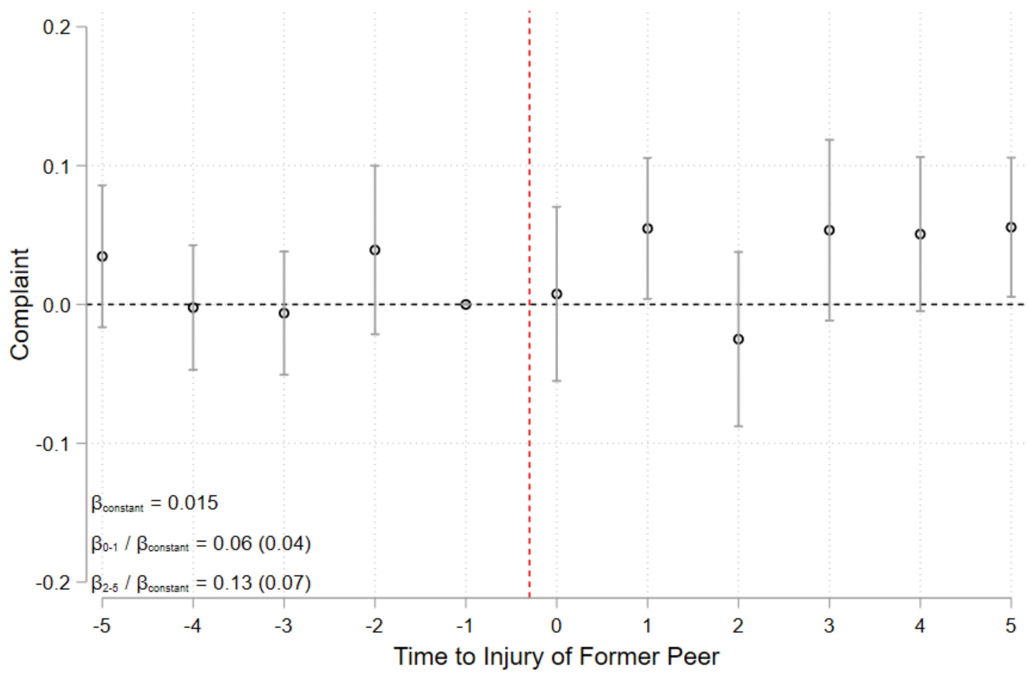
Note: The graph shows coefficient estimates using Equation 3 divided by the baseline rate of force-use and 95% confidence intervals. The baseline rate of force is calculated as the constant term from a regression of force on treatment lags and leads without fixed effects. Standard errors clustered by academy cohort (G = 81). The regression includes individual and unit-week fixed effects. Treatment is an injury of a former peer of the same race. Red vertical line represents the injury week.

Figure 9: Effect of Low-Resistance Injuries to Former Peers on Force-Use



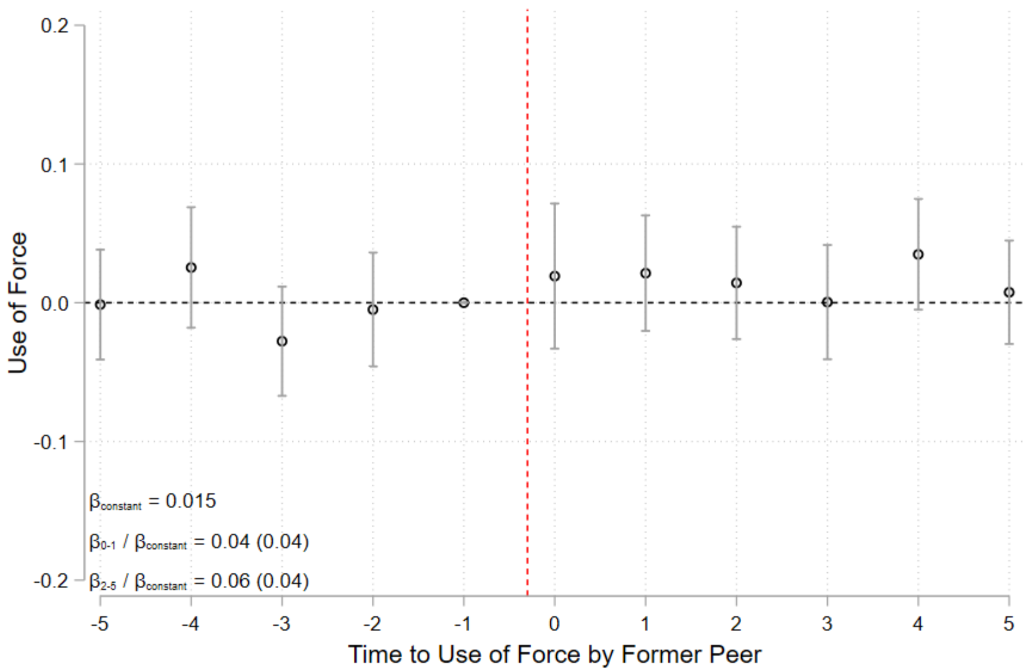
Note: The graph shows coefficient estimates using Equation 3 divided by the baseline rate of force-use and 95% confidence intervals. Events are injuries to former peers that occurred during interactions where the officer reported that the suspect used no resistance or non-violent resistance. The baseline rate of force is calculated as the constant term from a regression of force on treatment lags and leads without fixed effects. Standard errors clustered by academy cohort (G = 81). The regression includes individual and unit-week fixed effects. Treatment is an injury of a former peer. Red vertical line represents the injury week.

Figure 10: The Effect of Former Peer Injuries on Complaints



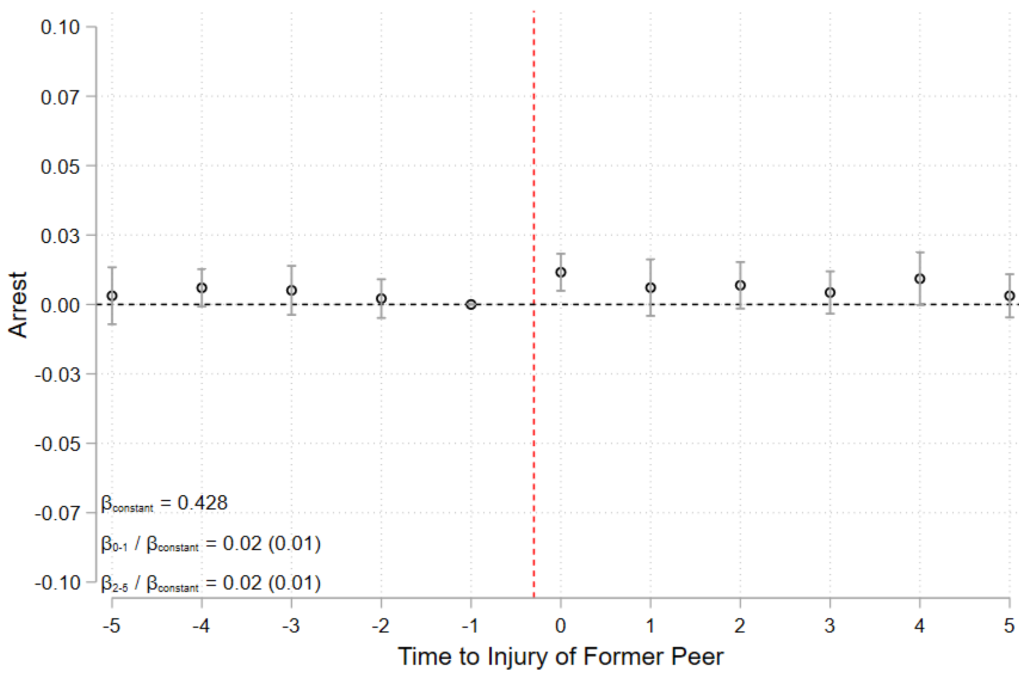
Note:The graph shows coefficient estimates using Equation 3 divided by the baseline rate of complaints and 95% confidence intervals. The baseline rate of complaints is calculated as the constant term from a regression of complaints on treatment lags and leads without fixed effects. Standard errors clustered by academy cohort (G = 81). The regression includes individual and unit-week fixed effects. Treatment is an injury of a former peer. Red vertical line represents the injury week.

Figure 11: The Effect of Former Peer Force-Use on Officer Force



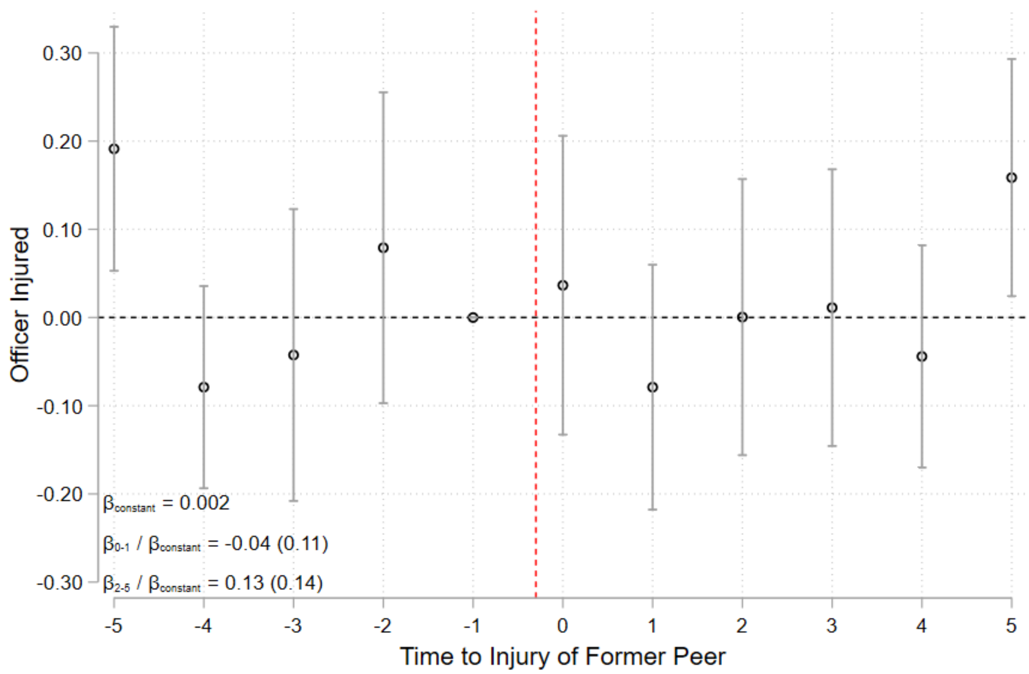
Note: The graph shows coefficient estimates using Equation 3 where the outcome is the use of force and the event is the use of force of former peers who did not experience an injury. Coefficients are divided by the baseline rate of force and 95% confidence intervals. The baseline rate of force is calculated as the constant term from a regression of force on treatment lags and leads without fixed effects. Standard errors clustered by academy cohort (G = 81). The regression includes individual and unit-week fixed effects. Treatment is an injury of a former peer. Red vertical line represents the injury week.

Figure 12: The Effect of Former Peer Injuries on Arrests



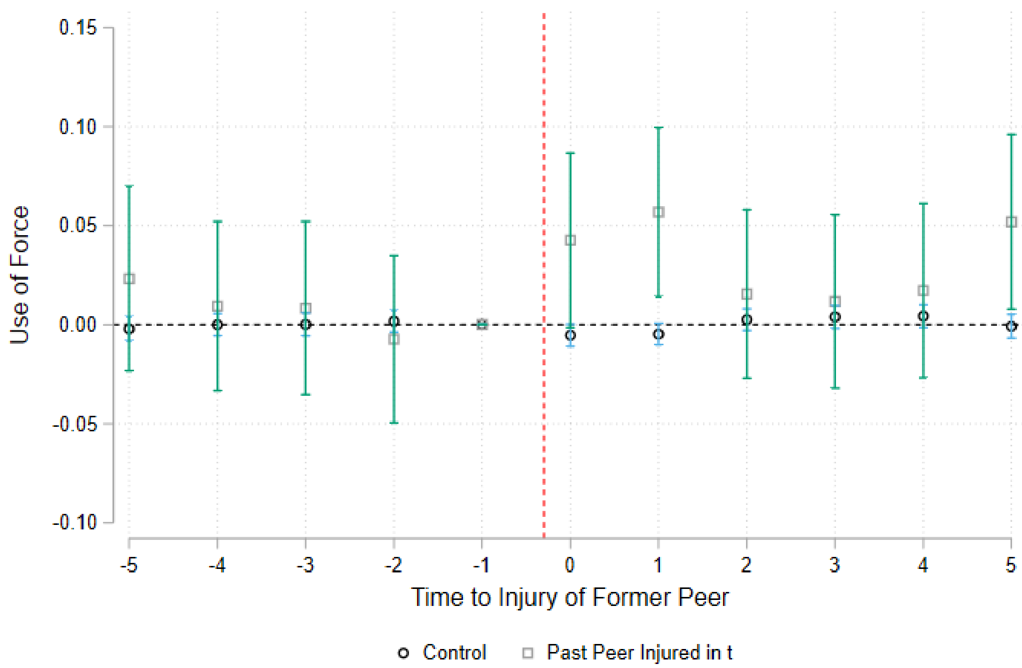
Note: The graph shows coefficient estimates using Equation 3 divided by the baseline rate of arrests and 95% confidence intervals. The baseline rate of arrests is calculated as the constant term from a regression of arrests on treatment lags and leads without fixed effects. Standard errors clustered by academy cohort (G = 81). The regression includes individual and unit-week fixed effects. Treatment is an injury of a former peer. Red vertical line represents the injury week.

Figure 13: The Effect of Former Peer Injuries on Own Injuries



Note: The graph shows coefficient estimates using Equation 3 divided by the baseline rate of officer injuries and 95% confidence intervals. The baseline rate of officer injuries is calculated as the constant term from a regression of FPS complaints on treatment lags and leads without fixed effects. Standard errors clustered by academy cohort (G = 81). The regression includes individual and unit-week fixed effects. Treatment is an injury of a former peer. Red vertical line represents the injury week.

Figure 14: The Effect of Former Peer Injuries on Use of Force



Note: This figure displays the propensity to use force in periods around officer injuries relative to the week before an officer injury. The control group refers to individuals who did not have a former peer injured. The values are calculated by first regressing force-use on individual and unit week fixed effects. The residuals from this regression are then averaged within indicators representing periods of time since an officer injury. We also present 95% confidence intervals using standard errors clustered at the individual level.

A. Appendix

A.1 Supplementary Tables

Table A1: Police Entrance Lotteries

Exam	Dates of Administration	Attended	Passed	Classes	Officers
2000-2002	No information	No information	No information	9	194
2002	1/12/2002	3,150	No information	16	268
2003	11/22/2003	No	No information	4	24
2004	11/20/2004	4,163	No information	7	317
2005	2/18/2006; 2/19/2006	4,061	3,338	3	173
2006-1	6/4/2006	1,508	1,255	2	139
2006-2	8/6/2006	1,025	863	3	181
2006-3	11/5/2006	1,795	1,487	14	806
2010	12/11/2010 makeups: 3/12/2011; 6/11/2011; 9/25/2011; 12/3/2011; 6/2/2013; 12/1/2012; 3/9/2013	8,621	7,689	22	1227
2013	12/14/2013 military makeups 6/28/2014; 12/7/2014; 6/13/2015; 12/6/2015	14,788	12,877	1	139

Note: Sample includes every officer who started at the police academy between January 2000 and December 2013. Cohorts who joined after December 13, but took the 2013 test are excluded because they do not have TRR data after their probationary period ends.

Table A2: Predictive Power of Lagged Outcomes on Treatment

	(1) Former Peer Injured	(2) Former Peer Injured	(3) Former Peer Injured	(4) Former Peer Injured	(5) Former Peer Injured
Outcome in $t-1$	-0.0102	-0.00130	-0.00722*	0.00108	0.00397
	(0.00753)	(0.00217)	(0.00407)	(0.000790)	(0.00261)
Outcome in $t-2$	0.00902	-0.00159	-0.00171	0.000172	0.00500
	(0.00832)	(0.00238)	(0.00383)	(0.00106)	(0.00307)
Outcome in $t-3$	-0.00354	0.00111	0.00243	-0.000693	-0.000107
	(0.00801)	(0.00268)	(0.00477)	(0.00102)	(0.00235)
Outcome in $t-4$	-0.00614	0.000827	-0.00750	0.000631	-0.000278
	(0.00512)	(0.00202)	(0.00455)	(0.000816)	(0.00221)
Outcome in $t-5$	0.0175**	0.00271	0.00846	-0.000368	0.00338
	(0.00695)	(0.00254)	(0.00560)	(0.00108)	(0.00247)
Outcome in $t-6$	0.00669	0.00300	0.00436	0.00150	-0.00454*
	(0.00574)	(0.00239)	(0.00440)	(0.000981)	(0.00272)
Constant	0.113***	0.113***	0.113***	0.115***	0.113***
	(0.0000390)	(0.000121)	(0.0000630)	(0.000622)	(0.0000717)
Lagged Outcome	Officer Injured	Force	Suspect Injury	Arrest	Complaint
Unit-Week Fixed Effects	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES
R-squared	0.121	0.121	0.121	0.119	0.121
Observations	967104	967104	967104	915399	967104

Note: Each column displays the results of a linear regression regressing six lags of the specified outcome on whether the officer has a former peer injured. We cluster standard errors on the police academy cohort level (G = 81). The F test row displays the F test for the joint equality of all lags for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

Table A3: Predictive Power of Characteristics on Treatment

	(1) Former Peer Injured	(2) Former Peer Injured	(3) Former Peer Injured	(4) Former Peer Injured	(5) Former Peer Injured
Officer is Female	-0.000839	0.000116	0.000128	0.000415	0.000844
	(0.00314)	(0.00300)	(0.00275)	(0.00194)	(0.000951)
Officer is Black	0.0107***	0.00462	0.00341	-0.00202	0.000254
	(0.00260)	(0.00322)	(0.00326)	(0.00204)	(0.00106)
Officer is Hispanic or Other	0.00337	0.00317	0.00278	-0.00296*	0.0000591
	(0.00273)	(0.00250)	(0.00238)	(0.00152)	(0.000878)
Officer Age at Test	-0.00463***	-0.00439***	-0.00392***	-0.000111	-0.0000217
	(0.000386)	(0.000367)	(0.000502)	(0.000137)	(0.0000653)
Portion of Cohort that is Male	0.0947	0.0898	0.0865	0.00813	0.0164
	(0.0913)	(0.0876)	(0.0800)	(0.0609)	(0.0270)
Portion of Cohort that is Black	0.0418	0.0375	0.0169	-0.0181	0.00319
	(0.0773)	(0.0741)	(0.0707)	(0.0653)	(0.0271)
Portion of Cohort that is Hispanic or Other	-0.0482	-0.0503	-0.0550	-0.123**	-0.0307
	(0.0802)	(0.0760)	(0.0697)	(0.0530)	(0.0232)
Average Age of Cohort	-0.00771	-0.00731	-0.00731	-0.00511	-0.00418**
	(0.00517)	(0.00506)	(0.00486)	(0.00421)	(0.00171)
Constant	0.415**	0.402**	0.394**	0.294**	0.224***
	(0.184)	(0.179)	(0.163)	(0.130)	(0.0534)
Unit Fixed Effects	NO	YES	NO	NO	NO
Unit-Week Fixed Effects	NO	NO	YES	YES	YES
Test Fixed Effects	NO	NO	NO	YES	YES
Number of Past Peers Fixed Effects	NO	NO	NO	NO	YES
R-squared	0.009	0.010	0.083	0.098	0.117
Observations	989908	989908	989885	989885	989885

Note: Each column displays the results of a linear regression regressing officer and cohort characteristics on whether the officer has a former peer injured. We cluster standard errors on the police academy cohort level (G = 81). $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

Table A4: Main results Poisson Specification

	(1) Force	(2) Injure Suspect	(3) Arrest	(4) Complaint	(5) Officer Injured	(6) Force
Former peer injured in t+6 or earlier	0.01103	0.02600	0.00363	-0.04630	0.08210	0.03019
	(0.02240)	(0.04210)	(0.00382)	(0.02833)	(0.05863)	(0.03215)
Former peer injured in t+5	0.00665	0.05046	0.00081	0.02757	0.15662	0.00946
	(0.02443)	(0.04724)	(0.00545)	(0.02547)	(0.06044)	(0.02880)
Former peer injured in t+4	-0.00508	-0.09331	0.00317	-0.00408	-0.08041	0.02503
	(0.01937)	(0.04516)	(0.00360)	(0.02274)	(0.05082)	(0.02686)
Former peer injured in t+3	0.00337	0.00420	0.00146	-0.00868	-0.04439	-0.00769
	(0.02445)	(0.04645)	(0.00466)	(0.02119)	(0.07918)	(0.03072)
Former peer injured in t+2	-0.02234	-0.02775	-0.00089	0.03692	0.08421	-0.02097
	(0.02460)	(0.04123)	(0.00383)	(0.03002)	(0.08349)	(0.03459)
Former peer injured in t+1	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000
	(.)	(.)	(.)	(.)	(.)	(.)
Former peer injured in t	0.02787	0.07892	0.00888	0.00270	0.04087	0.01220
	(0.02252)	(0.03823)	(0.00349)	(0.03131)	(0.07575)	(0.03470)
Former peer injured in t-1	0.05315	0.09058	0.00229	0.05128	-0.07105	0.11209
	(0.02635)	(0.04609)	(0.00529)	(0.02535)	(0.07194)	(0.03621)
Former peer injured in t-2	0.00823	0.06416	0.00289	-0.02958	-0.01143	0.04974
	(0.01869)	(0.03968)	(0.00439)	(0.02990)	(0.07361)	(0.03178)
Former peer injured in t-3	0.00002	0.02829	0.00126	0.04180	0.00414	-0.02150
	(0.02399)	(0.04428)	(0.00395)	(0.03174)	(0.08026)	(0.02454)
Former peer injured in t-4	0.01056	-0.02535	0.00624	0.04858	-0.07655	0.00685
	(0.02646)	(0.04249)	(0.00470)	(0.02759)	(0.06125)	(0.03077)
Former peer injured in t-5	0.04251	0.05445	0.00025	0.05479	0.14261	0.00642
	(0.02620)	(0.05475)	(0.00403)	(0.02623)	(0.05426)	(0.03297)
Former peer injured in t-6 or later	0.10659	0.25887	0.13277	0.11366	0.28429	0.12059
	(0.05147)	(0.08216)	(0.02525)	(0.06128)	(0.12612)	(0.03688)
Constant	-3.11795	-3.45868	-0.86579	-3.23269	-3.42101	-3.11464
	(0.04880)	(0.07898)	(0.02422)	(0.06018)	(0.12313)	(0.03199)
Model	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson
Peer Definition	Former Peer	Former Peer	Former Peer	Former Peer	Former Peer	Same-Race Former Peer
Unit-Week Fixed Effects	YES	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES	YES
Pre-trend Test	0.840	0.143	0.860	0.593	0.072	0.749
Observations	576,943	218,500	899,596	463,227	82,545	576,943

Note: Columns (1) through (7) display coefficients from estimates of Equation 3 where the outcome variable is an indicator representing whether the officer used a specific type of force. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table A5: Heterogeneous Effects by Type of Force

	(1) Control	(2) No Weapon	(3) Non-Lethal	(4) Baton	(5) Taser	(6) Firearm	(7) Other
Former peer injured in t+6 or earlier	-0.00031	0.00054	0.00008	0.00003	0.00011	-0.00004	-0.00009
	(0.00034)	(0.00041)	(0.00009)	(0.00008)	(0.00014)	(0.00006)	(0.00013)
Former peer injured in t+5	0.00053	-0.00002	-0.00000	0.00009	-0.00012	0.00005	0.00021
	(0.00043)	(0.00046)	(0.00009)	(0.00007)	(0.00011)	(0.00008)	(0.00013)
Former peer injured in t+4	0.00023	0.00023	0.00014	0.00004	-0.00005	-0.00001	-0.00011
	(0.00037)	(0.00032)	(0.00010)	(0.00009)	(0.00013)	(0.00006)	(0.00013)
Former peer injured in t+3	0.00024	-0.00010	0.00005	0.00008	0.00002	-0.00002	0.00005
	(0.00046)	(0.00043)	(0.00007)	(0.00008)	(0.00016)	(0.00007)	(0.00010)
Former peer injured in t+2	-0.00006	-0.00027	-0.00021	-0.00003	0.00017	0.00001	0.00005
	(0.00035)	(0.00040)	(0.00009)	(0.00007)	(0.00013)	(0.00006)	(0.00014)
Former peer injured in t+1	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Former peer injured in t	0.00036	0.00067	-0.00010	0.00010	0.00013	-0.00005	0.00008
	(0.00039)	(0.00050)	(0.00010)	(0.00008)	(0.00014)	(0.00007)	(0.00010)
Former peer injured in t-1	0.00070	0.00093	-0.00002	-0.00003	0.00004	0.00012	0.00008
	(0.00038)	(0.00050)	(0.00008)	(0.00007)	(0.00014)	(0.00007)	(0.00015)
Former peer injured in t-2	0.00008	0.00010	-0.00003	0.00008	-0.00034	0.00004	-0.00007
	(0.00034)	(0.00037)	(0.00011)	(0.00007)	(0.00012)	(0.00006)	(0.00014)
Former peer injured in t-3	0.00010	0.00032	0.00003	-0.00013	0.00000	-0.00007	0.00005
	(0.00034)	(0.00042)	(0.00012)	(0.00008)	(0.00012)	(0.00004)	(0.00014)
Former peer injured in t-4	0.00013	0.00009	0.00016	0.00002	0.00005	-0.00013	-0.00001
	(0.00038)	(0.00044)	(0.00009)	(0.00007)	(0.00015)	(0.00004)	(0.00011)
Former peer injured in t-5	0.00071	0.00088	0.00006	0.00001	0.00030	0.00003	0.00000
	(0.00044)	(0.00054)	(0.00010)	(0.00009)	(0.00019)	(0.00007)	(0.00009)
Former peer injured in t-6 or later	-0.00041	0.00126	0.00034	-0.00022	0.00043	0.00003	-0.00030
	(0.00098)	(0.00104)	(0.00024)	(0.00020)	(0.00028)	(0.00014)	(0.00029)
Constant	0.01046	0.01292	0.00054	0.00068	0.00126	0.00026	0.00134
	(0.00094)	(0.00100)	(0.00022)	(0.00019)	(0.00028)	(0.00014)	(0.00027)
Unit-Week Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Pre-trend Test	0.684	0.742	0.010	0.407	0.495	0.923	0.545
R-squared	0.035	0.039	0.030	0.022	0.025	0.022	0.023
Observations	944,356	944,356	944,356	944,356	944,356	944,356	944,356

Note: Columns (1) through (7) display coefficients from estimates of Equation 3 where the outcome variable is an indicator representing whether the officer used a specific type of force. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table A6: Effect of Low-Resistance Injuries to Former Peers on Force-Use

	(1)	(2)
	Force	Force
Former peer injured in t+6 or earlier	-0.00036	-0.02876
	(0.00060)	(0.03076)
Former peer injured in t+5	0.00003	-0.00153
	(0.00075)	(0.03838)
Former peer injured in t+4	0.00036	0.02300
	(0.00057)	(0.02807)
Former peer injured in t+3	-0.00086	-0.04163
	(0.00071)	(0.03578)
Former peer injured in t+2	0.00108	0.05010
	(0.00061)	(0.02761)
Former peer injured in t+1	0.00000	0.00000
	(.)	(.)
Former peer injured in t-1	0.00034	0.01880
	(0.00054)	(0.02754)
Former peer injured in t-2	0.00019	0.01413
	(0.00058)	(0.02765)
Former peer injured in t-3	0.00003	0.00655
	(0.00057)	(0.02831)
Former peer injured in t-4	0.00062	0.03103
	(0.00053)	(0.02413)
Former peer injured in t-5	-0.00013	-0.00265
	(0.00090)	(0.04425)
Former peer injured in t-6 or later	0.00133	0.12552
	(0.00097)	(0.04581)
Constant	0.01636	-3.12012
	(0.00088)	(0.04241)
Model	OLS	Poisson
Unit-Week Fixed Effects	YES	YES
Individual Fixed Effects	YES	YES
Pre-trend Test	0.113	0.159
R-squared	0	
Observations	944,356	576,943

Note: Column (1) displays coefficients from estimates of Equation 3 where the outcome variable is an indicator representing whether the officer used force and the event is whether the officer's former peer used force but was not injured in the previous week. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table A7: Effect of Former Peer Injuries on Complaints Against Officers

	(1) All Complaints	(2) Force and Verbal	(3) Arrest and Search	(4) Failure to Provide Service	(5) Unbecoming Conduct
Former peer injured in t+6 or earlier	-0.00069	-0.00024	-0.00061	0.00032	0.00000
	(0.00041)	(0.00024)	(0.00024)	(0.00019)	(0.00005)
Former peer injured in t+5	0.00052	0.00007	0.00026	0.00016	0.00000
	(0.00038)	(0.00025)	(0.00020)	(0.00020)	(0.00006)
Former peer injured in t+4	-0.00003	-0.00013	0.00021	0.00000	-0.00004
	(0.00034)	(0.00018)	(0.00027)	(0.00014)	(0.00005)
Former peer injured in t+3	-0.00009	0.00003	0.00005	-0.00016	0.00005
	(0.00033)	(0.00022)	(0.00028)	(0.00016)	(0.00005)
Former peer injured in t+2	0.00059	0.00022	0.00015	0.00010	-0.00007
	(0.00046)	(0.00019)	(0.00034)	(0.00014)	(0.00005)
Former peer injured in t+1	0.00000	0.00000	0.00000	0.00000	0.00000
	(.)	(.)	(.)	(.)	(.)
Former peer injured in t	0.00011	0.00019	-0.00012	-0.00006	-0.00002
	(0.00047)	(0.00021)	(0.00029)	(0.00017)	(0.00005)
Former peer injured in t-1	0.00082	-0.00016	0.00044	0.00038	0.00002
	(0.00038)	(0.00020)	(0.00020)	(0.00018)	(0.00006)
Former peer injured in t-2	-0.00037	-0.00021	-0.00026	-0.00023	0.00015
	(0.00047)	(0.00022)	(0.00022)	(0.00017)	(0.00006)
Former peer injured in t-3	0.00080	0.00034	0.00040	0.00009	-0.00004
	(0.00049)	(0.00024)	(0.00019)	(0.00017)	(0.00005)
Former peer injured in t-4	0.00076	0.00034	0.00031	0.00002	-0.00004
	(0.00042)	(0.00017)	(0.00023)	(0.00015)	(0.00006)
Former peer injured in t-5	0.00083	0.00027	0.00020	0.00012	0.00001
	(0.00038)	(0.00016)	(0.00030)	(0.00018)	(0.00006)
Former peer injured in t-6 or later	0.00151	0.00071	0.00132	-0.00076	-0.00012
	(0.00096)	(0.00047)	(0.00049)	(0.00053)	(0.00015)
Constant	0.01157	0.00265	0.00374	0.00321	0.00038
	(0.00095)	(0.00044)	(0.00049)	(0.00050)	(0.00015)
Unit-Week Fixed Effects	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES
Pre-trend Test	0.412	0.516	0.117	0.468	0.550
R-squared	0.040	0.034	0.038	0.028	0.027
Observations	944,356	944,356	944,356	944,356	944,356

Note: Columns (1) through (5) display coefficients from estimates of Equation 3 where the outcome variable is an indicator representing types of complaints against the officer. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table A8: Heterogeneous Effects by Tenure

	(1) Force	(2) Injure Suspect	(3) Arrest	(4) Officer Injured	(5) Complaint
Former peer in previous week $\times$ Tenure (months)	-0.00004	-0.00002	-0.00008	0.00000	-0.00002
	(0.00002)	(0.00001)	(0.00005)	(0.00001)	(0.00001)
Former peer in previous week	0.00375	0.00175	0.00866	-0.00025	0.00227
	(0.00136)	(0.00078)	(0.00403)	(0.00038)	(0.00078)
Constant	0.01753	0.00537	0.37588	0.00236	0.01308
	(0.00006)	(0.00003)	(0.00025)	(0.00002)	(0.00004)
Model	OLS	OLS	OLS	OLS	OLS
Unit-Week Fixed Effects	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES
Pre-trend Test	NO	NO	NO	NO	NO
R-squared	0.742	0.159	0.493	0.070	0.491
Observations	0.040	0.031	0.262	0.026	0.039
N	986088	986088	953262	986088	986088

Note: Columns (1) through (5) display coefficients from estimates of Equation 2 with various indicators and an interaction term between a lagged injury to a former peer and the officer tenure. Officer tenure is a continuous variable representing the number of months since the officer started at the police academy. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table A9: Heterogeneous Effects by Number of Past Events

	(1) Force	(2) Injure Suspect	(3) Arrest	(4) Officer Injured	(5) Complaint
Former peer in previous week $\times$ Number of Previous (months)	-0.00006	-0.00003	-0.00005	-0.00000	-0.00002
	(0.00002)	(0.00001)	(0.00008)	(0.00001)	(0.00002)
Former peer in previous week	0.00272	0.00142	0.00521	-0.00010	0.00132
	(0.00091)	(0.00055)	(0.00297)	(0.00025)	(0.00058)
Constant	0.01755	0.00537	0.37589	0.00236	0.01309
	(0.00006)	(0.00003)	(0.00025)	(0.00002)	(0.00004)
Model	OLS	OLS	OLS	OLS	OLS
Unit-Week Fixed Effects	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES
Pre-trend Test	NO	NO	NO	NO	NO
R-squared	0.742	0.159	0.493	0.070	0.491
Observations	0.040	0.031	0.262	0.026	0.039
N	986,088	986,088	953,262	986,088	986,088

Note: Columns (1) through (5) display coefficients from estimates of Equation 2 with various indicators and an interaction term between a lagged injury to a former peer and the number of previous events. Number of previous events is a continuous variable representing the number of times the officer has experienced an injury to a former peer. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table A10: Heterogeneous Effects by Suspect Characteristics

	(1) White Suspect	(2) Minority Suspect	(3) Male Suspect	(4) Female Suspect
Former peer injured in t+6 or earlier	-0.00001	0.00041	0.00061	-0.00013
	(0.00012)	(0.00043)	(0.00048)	(0.00019)
Former peer injured in t+5	0.00006	0.00033	0.00031	0.00003
	(0.00012)	(0.00049)	(0.00047)	(0.00014)
Former peer injured in t+4	0.00006	-0.00010	0.00002	0.00003
	(0.00012)	(0.00037)	(0.00038)	(0.00017)
Former peer injured in t+3	0.00010	0.00011	0.00016	-0.00005
	(0.00010)	(0.00047)	(0.00047)	(0.00014)
Former peer injured in t+2	-0.00007	-0.00024	-0.00022	-0.00010
	(0.00008)	(0.00047)	(0.00039)	(0.00018)
Former peer injured in t+1	0.00000	0.00000	0.00000	0.00000
	(.)	(.)	(.)	(.)
Former peer injured in t	0.00015	0.00053	0.00080	-0.00013
	(0.00009)	(0.00046)	(0.00045)	(0.00015)
Former peer injured in t-1	0.00009	0.00105	0.00112	0.00003
	(0.00012)	(0.00051)	(0.00058)	(0.00015)
Former peer injured in t-2	0.00005	0.00021	0.00039	-0.00009
	(0.00012)	(0.00034)	(0.00034)	(0.00016)
Former peer injured in t-3	-0.00013	0.00038	0.00021	-0.00001
	(0.00008)	(0.00045)	(0.00044)	(0.00018)
Former peer injured in t-4	-0.00013	0.00020	0.00014	-0.00001
	(0.00014)	(0.00044)	(0.00044)	(0.00016)
Former peer injured in t-5	0.00012	0.00081	0.00084	0.00016
	(0.00014)	(0.00056)	(0.00051)	(0.00014)
Former peer injured in t-6 or later	0.00006	0.00057	0.00044	-0.00007
	(0.00022)	(0.00120)	(0.00114)	(0.00037)
Constant	0.00103	0.01538	0.01433	0.00261
	(0.00021)	(0.00113)	(0.00110)	(0.00036)
Unit-Week Fixed Effects	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES
Pre-trend Test	0.627	0.864	0.890	0.984
R-squared	0.036	0.040	0.040	0.026
Observations	944,356	944,356	944,356	944,356

Note: Columns (1) through (3) display coefficients from estimates of Equation 3 where the outcome variable is an indicator representing whether the officer used a specific type of force against a suspect of a given race. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table A11: Effect of Former Peer Force-Use on Officer Force-Use

	(1)	(2)
	Force	Force
Former peer injured in t+6 or earlier	0.00027	0.00895
	(0.00033)	(0.01873)
Former peer injured in t+5	-0.00002	-0.00766
	(0.00030)	(0.01586)
Former peer injured in t+4	0.00039	0.01835
	(0.00033)	(0.02016)
Former peer injured in t+3	-0.00042	-0.02283
	(0.00030)	(0.01630)
Former peer injured in t+2	-0.00008	-0.00342
	(0.00031)	(0.01757)
Former peer injured in t+1	0.00000	0.00000
	(.)	(.)
Former peer injured in t	0.00029	0.00855
	(0.00040)	(0.02121)
Former peer injured in t-1	0.00032	0.02013
	(0.00032)	(0.01743)
Former peer injured in t-2	0.00022	0.01174
	(0.00031)	(0.01748)
Former peer injured in t-3	0.00001	-0.00353
	(0.00032)	(0.01696)
Former peer injured in t-4	0.00053	0.02889
	(0.00031)	(0.01714)
Former peer injured in t-5	0.00011	0.00705
	(0.00029)	(0.01561)
Former peer injured in t-6 or later	0.00165	0.12656
	(0.00205)	(0.09700)
Constant	0.01526	-3.16130
	(0.00211)	(0.10323)
Unit-Week Fixed Effects	YES	YES
Individual Fixed Effects	YES	YES
Pre-trend Test	0.561	0.621
R-squared	0.041	
Observations	944,356	576,943

Note: Column (1) displays coefficients from estimates of Equation 3 where the outcome variable is an indicator representing whether the officer used force and the event is whether the officer's former peer used force but was not injured in the previous week. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table A12: Effect of Former Peer Injuries on Officer Arrests

	(1) Any Arrest	(2) Non-Index Crime	(3) Property Crime	(4) Violent Crime
Former peer injured in t+6 or earlier	0.00267	0.00162	-0.00032	0.00189
	(0.00156)	(0.00150)	(0.00082)	(0.00128)
Former peer injured in t+5	0.00134	0.00047	0.00073	0.00139
	(0.00221)	(0.00149)	(0.00094)	(0.00092)
Former peer injured in t+4	0.00256	-0.00023	0.00154	0.00116
	(0.00145)	(0.00112)	(0.00100)	(0.00098)
Former peer injured in t+3	0.00218	0.00151	-0.00042	-0.00125
	(0.00190)	(0.00142)	(0.00074)	(0.00118)
Former peer injured in t+2	0.00090	-0.00058	0.00219	0.00102
	(0.00150)	(0.00135)	(0.00076)	(0.00136)
Former peer injured in t+1	0.00000	0.00000	0.00000	0.00000
	(.)	(.)	(.)	(.)
Former peer injured in t	0.00497	0.00258	0.00257	0.00177
	(0.00144)	(0.00124)	(0.00081)	(0.00099)
Former peer injured in t-1	0.00259	0.00297	0.00090	-0.00040
	(0.00219)	(0.00181)	(0.00104)	(0.00123)
Former peer injured in t-2	0.00295	0.00134	0.00117	0.00174
	(0.00180)	(0.00160)	(0.00105)	(0.00106)
Former peer injured in t-3	0.00184	0.00159	-0.00080	0.00023
	(0.00164)	(0.00159)	(0.00099)	(0.00093)
Former peer injured in t-4	0.00397	0.00396	0.00038	0.00263
	(0.00204)	(0.00134)	(0.00109)	(0.00134)
Former peer injured in t-5	0.00133	0.00097	0.00260	0.00035
	(0.00167)	(0.00163)	(0.00077)	(0.00127)
Former peer injured in t-6 or later	0.04608	0.06160	0.00033	-0.01181
	(0.01085)	(0.00595)	(0.00397)	(0.00623)
Constant	0.33539	0.16232	0.06322	0.11042
	(0.01042)	(0.00575)	(0.00384)	(0.00605)
Unit-Week Fixed Effects	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES
Pre-trend Test	0.423	0.675	0.052	0.106
R-squared	0.259	0.253	0.077	0.077
Observations	914,061	914,061	914,061	914,061

Note: Columns (1) through (8) display coefficients from estimates of Equation 3 where the outcome variable is an indicator representing arrests for various types of crime. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

Table A13: Effect of Former Peer Injuries on Officer Arrests (2010-2016)

	(1)	(2)	(3)	(4)
	Any Arrest	Non-Index Crime	Property Crime	Violent Crime
Former peer Injured in previous week	0.00414*	0.00401**	0.000583	0.000155
	(0.00244)	(0.00194)	(0.00121)	(0.00122)
Constant	0.348***	0.204***	0.0556***	0.0892***
	(0.000270)	(0.000215)	(0.000133)	(0.000135)
Model	OLS	OLS	OLS	OLS
Percent Increase	1.21	2	1.06	.18
Unit-Week Fixed Effects	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES
Pre-trend Test	.33	.565	.557	.065
R-squared	0.254	0.254	0.065	0.068
Observations	770,394	770,394	770,394	770,394

Note: Columns (1) through (8) display coefficients from estimates of Equation 2 where the outcome variable is an indicator representing arrests for various types of crime, replicating the results in Table 17 except only including observations after the 9th week of 2010 to avoid mismeasurement of the arrest date as discussed in Appendix A.2. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81).

Table A14: Effect of Injuries to Former Peers of the Same Gender

	(1)	(2)	(3)	(4)	(5)	(6)
	Force	Force	Force	Force	Force	Force
Same-race former peer injured in previous week	0.00671***	0.00565***	0.00422***	0.00362***	0.00138**	0.0512**
	(0.00101)	(0.000982)	(0.000815)	(0.000774)	(0.000642)	(0.0260)
Constant	0.0172***	0.0172***	0.0173***	0.0174***	0.0176***	-3.016***
	(0.000562)	(0.000556)	(0.000424)	(0.000352)	(0.0000502)	(0.00275)
Model	OLS	OLS	OLS	OLS	OLS	Poisson
Percent Increase	39.11	32.96	24.58	21.1	8.05	5.12
Unit-Week Fixed Effects	NO	YES	YES	YES	YES	YES
Number of Former Peers	NO	NO	YES	YES	NO	NO
Test Period Fixed Effects	NO	NO	NO	YES	NO	NO
Individual Fixed Effects	NO	NO	NO	NO	YES	YES
Pre-trend Test	0	0	0	0	.82	.99
R-squared	0.000	0.022	0.023	0.024	0.040	
Observations	986111	986088	986088	986088	986088	607808

Note: Column (1) displays estimates from a linear regression of an indicator for any force used by the officer on the first lag of injuries to same-gender former peers. Column (2) controls for unit-week fixed effects. Column (3) controls for unit-week and number of former peer fixed effects. Column (4) controls for unit-week, number of former peers, and estimated test period fixed effects. Column (5) estimates Equation 2, controlling for individual and unit-week fixed effects. Column (6) estimates Equation 2 using Poisson maximum likelihood estimation. We calculate the percent increase by dividing the column's coefficient by the baseline in a regression without fixed effects. We cluster standard errors on the police academy cohort level (G = 81). The pre-trend test row presents the p-value from an F-test for which the null hypothesis is that the coefficients of six lead periods in the event-study specification are simultaneously equal to zero.

A.2 Construction of Data Set

To construct our treatment and outcome variables, we link administrative unit assignments from the Chicago Police Department to (i) tactical response reports created after a police officer uses force, (ii) arrest data generated after an officer arrests a suspect, and (iii) formal complaints against an officer.

This section describes the linking process and illustrates how we go from the original sample of officers, arrests, instances of force-use, and complaints to the sample we use for analysis.

A.2.1 Police Academy Cohorts

The administrative district assignments data ranges from before 2002 to December 2016. This data is combined with salary data (from 2002 to 2017) which importantly gives rank information. This data set contains the unit assignment of each office who served at any point during this period. Officers in the Recruit Training Unit (Unit 44) are part of the police academy for the first six months in that unit and on a probationary period during the following twelve months with some variation. We construct the final sample if 3,491 officers from the full sample of 29,894 officers by doing the following:

1. We drop 24,533 officers who graduated from the probationary period before January 2002 (24,533) because they are not present in the salary data for their entire careers. Also the lottery based system was only introduced in the early 1990's, and records of test dates (used to impute cohort test groups) begin in January 2002.

2. We impute the start month for 196 officers who graduate from the police academy within a year of January 2002 but begin the police academy before January 2002.

3. We drop 973 officers who never leave the Recruit Training Unit during the sample period. The dropped officers include 645 officers who begin at the academy in May 2015 or later and 328 officers who start before May 2015.

4. We drop 23 officers who start in the same month with three or fewer other officers because we believe these to be errors.

We restrict the sample to officers who enter one of 25 geographic districts after graduating from their probationary period. This means that we drop non-standard units such as the canine unit or S.W.A.T. team, who move between geographic districts from day to day. We also drop officers who leave the police academy before six months or individuals who never are registered as leaving the police academy in our sample. We cannot link these data to academy cohorts or the use of force data and cannot be used in the analysis. We also drop thirty-three individuals who have cohort start dates with five or fewer people.

A.2.2 Unit Assignments

After restricting the data to 3,491 officers, we then match officers to police districts using a monthly unit-assignment panel based on unit assignment data obtained from the Chicago Police Department. These assignments tell us the unit assignment of each officer throughout the sample period.

The geographic unit assignments begin roughly eighteen months after a police officer enrolls at the academy. We throw out any months where an officer works in a unit that does not have geographic boundaries. These units include the SWAT team, bomb squad, canine units, detectives, etc.. Out of the

3,491 officers in the data set, 3,468 spend at least one month in a geographic unit, and ninety-one percent of days are spent in geographic units.

A.2.3 Tactical Response Reports

The primary source of data comes from the Chicago Police Department's Tactical Response Reports (TRR). The CPD requires that officers fill out a TRR after instances of force-use under circumstances that appear in the CPD's use-of-force model. We use data from TRRs filed between January 8th, 2004 to October 31st, 2016. For every week in the data set, we use this data to measure whether officers use any force in a given week. We also use this data to measure the highest level of force the officers choose to use in that week, if any. Finally, this data is used to measure whether officers or suspects sustain any injuries during their encounters.

A TRR must be filed after use of force incidents involving subjects classified as active resisters or assailants. However, some exceptions apply when actively resisting suspects are fleeing, and the members are restricted to verbal commands and/or control holds in conjunction with handcuffing and searching techniques that do not result in the allegation of an injury. For subjects classified as cooperative or passive resisters, police must fill out a TRR if the subject is injured or alleges an injury. A TRR must also be filed for all incidents where a subject obstructs a police officer (Chicago Police Department 2016).

All TRR's require a supervisor's approval. The supervisor must notify the external oversight agency for incidents involving the use of deadly force or the discharge of a firearm, Taser, pepper spray, or other chemical weapons. An external oversight agency must also be notified after an allegation of excessive force.

The variables in the dataset include the date of the incident, number of involved officers, injured officers, suspects' race and ethnicity, injured suspects, and the type of force used against the suspect. One limitation of our dataset is that it includes no records for incidents involving juvenile suspects or suspects with unknown ages.

We classify use of force incidents into six categories according to the highest type of force used in the incident. Our type of force hierarchy comes from the CPD use of force model. No and minor force are the only types of force that are authorized for compliant or passively resistant subjects. As mentioned above, TRR's are not required for such incidents unless the subject is injured. We suspect that many incidents involving minor force or less that do not result in injuries are unreported.

Police report injuries to police officers or suspects; the TRR asks explicitly whether the police officer injured the subject. The observed injury rates may reflect some combination of reporting requirements and voluntary reporting.

A.2.4 Arrest Data

Next, we merge this data set with data on arrests made by every officer during this sample period. For every officer, this data set includes every arrest that the officer makes of suspects who are 18 years of age or older. The City of Chicago prevents the disclosure of information on the arrests of juvenile suspects. These suspects are, therefore, excluded from the analysis.

We restrict the sample of arrests to the same period as the TRRs (January 8th, 2004 to October 31st, 2016). Arrest dates are only available from 2010 to 2017. For all years in this study before 2010, we impute the arrest date as the earlier of the bond and release date. Between 2010 and 2017, the median number of days between arrest date and the earliest of the bond and release date is one day (the average is 0.71 days).

For all of the sample's arrests, the arrest data contains a crime code, which describes the reason for the arrest. These codes can designate an arrest for a violent crime, property crime, traffic violation, outstanding warrant, drug crime, municipal code violation, or other violation.

A.2.5 Complaint Data

The complaint data contains all recorded allegations of misconduct filed against officers from 2000 to 2016. The allegations can come from either another officer or a civilian. Each complaint contains information on the officer, complainant demographics, and the date of the incident.

We merge this data to the unit assignment data to measure whether an officer received a complaint about any incident during a given week. We are also able to measure the nature of the complaint. For more information about the complaint data, see Ba (2017).

A.3 Background on Lottery

Becoming a Chicago Police officer is a highly sought after career, with thousands of applicants taking the initial entrance exam, which are offered every few years over the past two decades (see Table A1). The practice of determining which applicants may enter the police academy based on random lottery number began in the early 1990's as a part of Mayor Daley's attempt to meet racial hiring quotas. This proposal was met with significant uproar and criticism at the time. From one Chicago Tribune article at the time: "Daley came under fire again because new police recruits are being chosen by lottery, not by their performances on the department's entrance exam...The computer then blindly arranged [qualified candidates] in a hiring order that had nothing to do with test results, the officials said" Blau and Kass (2018). From this and other available information, the process is straightforward:

1. Applicants take the test.
2. Passing applicants are given a lottery number randomly generated by a computer.
3. Passing applicants, who are eligible to join the academy, are permitted to enter the academy in order after passing psychological, medical, and physical examinations.

The random lottery process is now accepted by applicants and a common feature of CPD's FAQs on applying to the department, as the 2018 FAQs state: "All applicants who pass the exam are placed on an eligibility list, and that list is sorted in lottery order. You will be referred to the Chicago Police Department in lottery order as vacancies become available" (Department, 2018). The City of Chicago also uses lottery numbers for training to become a firefighter and EMT (CFD, 2014).

This practice is also noted in multiple news articles (Pritchard, 2013; NBC, 2013) and by the Chicago Inspector General (OIG, 2016). While the exact conditions for being drawn in have changed in recent years (after the 2013 test, 21 year-old's were eligible, and preference considerations were made for certain groups such as veterans), two features have remained constant for almost 30 years: lottery ordering and significantly more eligible applicants than spots in the CPD.

Unfortunately, which recruits belong to which cohorts is not able to be obtained through the FOIA to the CPD. A request made in August of 2020 for: ”A file containing the date of the test which each officer appointed between 2000 and 2020 took... A file containing the date at which each entrance exam's eligible officer list was retired.” was not fulfilled due to excessive burden and noted that ”... the Chicago Police Department simply may not compile or maintain in entirety or with the level of detail or sub-categorization you seek...” (FOIA P589445). Based on all available documentation and data, there is no reason to believe a list of eligible applicants is retired when a new test is issued. Rather, it appears to take many months, if not a year, for the first applicants to have their numbers called following a test. This means identifying which cohorts belong to which test with certainty for the breadth of our data is not feasible.

Summary statistics for these entrance lotteries appears in table A1. On average, 85% of test takers pass the entrance exam and 20% of these enter the police academy, based on proxy test dates discussed in the main text.[35] We evaluate the balance of the lotteries by performing a multinomial logisitic regression of start month group on the police officers' age, race, and sex. We then use a chi-squared test to determine whether any of the characteristics can predict entrance to a certain police academy cohort. There appears to be some imbalance in two of the nine test-cohorts. This imbalance would be concerning if we were explicitly looking at the effect of contextual effects in police force. However, since the empirical strategy uses a difference-in-

[35]There is substantial heterogeneity in the portion of eligible people who enter the academy, ranging from 3% in 2013 to 64% in the first 2006 exam.

differences design the imbalance in these two cohorts will not bias the treatment estimates.

A.3.1 Waiting List

Academy cohorts being constructed through a waiting list may raise concerns over identification of treatment effects, as discussed in de Chaisemartin and Behaghel (2020). However, their paper discusses the issues associated with treatment being assigned based on randomized waiting lists, where demand for treatment is oversubscribed and treatment effects are estimated by comparing those who received treatment to those who did not. While the CPD academy assignment process is based on lottery numbering waitlists, with far more eligible applicants than spots available, the treatment effect analogous to those discussed in (de Chaisemartin and Behaghel, 2020) is the effect of becoming a Chicago Police Officer (e.g., comparing economic or social outcomes of applicants who entered the academy and those who did not).

In this paper, the population of interest is Chicago Police officers and the randomly assigned academy cohorts are known peer-groups with whom injured officers have social ties but do not experience the same local crime shocks. All of our results are conditional on one being a CPD officer and our control group is not applicants to the department who never had their number drawn. While applying (de Chaisemartin and Behaghel, 2020) to a study of the effect of becoming a police officer would be appropriate, it is not applicable in our environment. Furthermore, the CPD does not provide any information on applicants who did not enter the academy, actual lottery numbers, or waitlists, so constructing a corrected estimator would not be possible.